THURSDAY, OCTOBER 7, 1875

THE ASTRONOMY OF THE BABYLONIANS

THE astronomical science of the ancient Babylonians and their pupils, the Assyrians, was neither so profound nor so contemptible as has often been maintained. Now that we are able to read the native records written in the cuneiform or wedge-shaped character, we find that the progress made at a very early period in mapping out the sky, in compiling a calendar, and above all in observing the phenomena of the heavens, was really wonderful, considering the scanty means they possessed of effecting it. Certainly their astronomy was mixed up with all kinds of astrological absurdities, but this did not prevent them from being persistent and keen observers, whose energy in the cause of knowledge is not undeserving of imitation even in the present day.

The originators of astronomy in Chaldea, as indeed of all other science, art, and culture there, were not the Semitic Babylonians, but a people who are now generally termed Accadians, and who spoke an agglutinative language. They had come from the mountains of Elam or Susiana, on the east, bringing with them the rudiments of writing and civilisation. They found a cognate race already settled in Chaldea, and in conjunction with the latter they built the great cities of Babylonia, whose ruins still attest their power and antiquity. Somewhere between 3000 and 4000 B.C. the Semites entered the country from the east, and gradually contrived to conquer the whole of it. It is probable the conquest was completed about 2000 B.C. At all events, Accadian became a dead language two or three centuries later, but as the Semitic invaders owed almost all the civilisation they possessed to their more polished predecessors, it remained the language of literature, like Latin in the Middle Ages, down to the last days of the Assyrian Empire.

Astronomy was included in the branches of science borrowed by the Semitic Babylonians from the Accadians. Consequently their astronomical records contain many words which belong to the old language, while most of the stars bear Accadian and not Semitic names. Even where the Assyro-Babylonians had a technical term of their own, like *kasritu*, "conjunction," they continued to *write* the old Accadian word *ribanna*, of which *kasritu* was a translation, though they probably *pronounced* it *kasritu*, just as we pronounce *viz.* "namely."

The oldest Chaldean astronomical records of which we know are contained in a great work called "The Observations of Bel," in 70 books, compiled for a certain King Sargon of Agané, in Babylonia, before 1700 B.C., and of which we possess later copies or editions, made for the Library of Sardanapalus at Nineveh. The catalogue of this work shows that a great part of it was purely astrological; other books, however, were more scientific. Thus there was one on the conjunction of the sun and moon, another on comets, or, as they are called, "stars with a corona in front and a tail behind," a third on the movements of Mars, a fourth on the movements of Venus, and a fifth on the Pole-star.* The catalogue concludes with a curious intimation to the student, who is told to write down the number of the tablet or book he wishes to consult, and the librarian will thereupon hand it to him. The larger portion of the work itself has been recovered, though some of the tablets belonging to it still lie under the soil of Kouyunjik, and a good part of the details which follow is extracted from this primitive Babylonian treatise.

* That is, α Draconis.

The Accadians seem to have begun their astronomical observations before they left Elam, since the meridian was placed in that country, while the old mythology made "the mountain of the East" the pivot on which the sky rested. This will account for the large number of eclipses recorded in the "Observations of Bel," which imply a corresponding antiquity for the commencement of such records. These records were carefully kept, as there were State Observatories in most of the Babylonian and Assyrian towns—at Ur, Agané, Nineveh, and Arbela, for instance—and (at all events in later times) the astronomers royal had to send fortnightly reports to the King.

It is to the Accadians that we owe both the signs of the Zodiac and the days of the week. The heaven was divided into four parts, and the passage of the sun through these marked the four seasons of the year. A tablet brought home by Mr. Smith informs us that the spring quarter lasted from the 1st of the month Adar to the 30th of the month Iyyar (that is, from the 1st degree of Pisces to the 30th degree of Taurus), the summer quarter from the 1st of Sivan to the 30th of Ab (the 1st degree of Gemini to the 30th of Leo), the autumn quarter from the 1st of Ebul to the 30th of Marchesvan (the 1st degree of Virgo to the 30th of Scorpio), and the winter quarter from the 1st of Chisleu to the 30th of Sebat (the 1st degree of Sagittarius to the 30th of Aquarius). The fact that the spring quarter did not commence with the beginning of the year in Nisan or March, shows that the scheme was subsequent to the formation of the calendar.

The year was divided into twelve lunar months and 360 days, an intercalary month being added whenever a certain star, called "the star of stars," or *Icu*,* which was just in advance of the sun when it crossed the vernal equinox, was not parallel with the moon until the 3rd of Nisan, that is, two days after the equinox. This, however, did not always suffice to keep the seasons in order, and the calendar had more than once to be rectified by the intercalation of other so-called months, consisting of a few days each. Cycles of twelve solar years were also in use, during which the same weather was expected to recur. The day was divided into twelve *casbumi*, or "double hours," each of these being further subdivided into sixty minutes and sixty seconds. The month, too, was cut into two halves of fifteen days, each subdivided into periods of five days, though a week of seven days was also employed from the earliest times. The days of the week were named after the sun, moon, and five planets; and since the 7th, 14th, 19th, 21st, and 28th of the month were termed "days of rest" on which certain works were forbidden to be done, it is clear that the origin of our modern week must be referred to the ancient Chaldeans. The names of the months were taken from the corresponding signs of the Zodiac, and as the Zodiac

* Called *Dil-gan*, or "messenger of light," in Accadian. It must be identified with τ Arietis, and at a later time with α Arietis.

began with Aries and the year with Nisan, neither the Zodiac nor the Calendar of the Accadians could be earlier than 2540 B.C. This is also indicated by the fact that even as late as the composition of the "Observations of Bel," time is calculated in the case of eclipses, not by the *casbu*, or "double hour"—a word which is Accadian, and not Semitic—but by the older division into three watches. These consisted of four hours each, beginning at 6 P.M. and ending at 6 A.M., and they were called respectively the "evening," "middle," and "morning" watches. Something like an accurate measurement of time was attained by the invention of the clepsydra.

Eclipses of the moon were observed from a very early epoch; but numerous as are the records of them in the great astronomical work of Sargon's Library, the vague and unscientific way in which they are recorded renders them of little value. The usual formula is: "In the month so and so, on the 14th day, an eclipse takes place, beginning on the east and ending on the west: it begins in the middle watch [10 P.M. to 2 A.M.], and ends in the morning watch, the shadow being eastward from the commencement to the cessation of the eclipse." In subsequent times, however, the language of the observatory reports becomes more precise and the gradual progress of an eclipse is carefully described. Long before the reign of Sargon of Agané, the discovery had been made that lunar eclipses recur after a cycle of 223 lunations, and records of them incorporated into the "Observations of Bel" generally begin with the words "According to calculation," or (it may be) "Contrary to calculation, the moon was eclipsed." One of the most curious tablets now in the British Museum is one of lunar longitudes, which seems to have formed part of the great Babylonian work on Astronomy, but, since it is written in Accadian, must be older than 2000 B.C. As a translation of it has not been made before, it is here given in full:—

1.	The 1st day (the moon) advances	...	...	5 deg.
2.	The 2nd day	,,	,,	10 deg.
3.	The 3rd day	,,	,,	20 deg.
4.	The 4th day	,,	,,	40 deg.
5.	The 5th day	,,	,,	80 deg.
6.	The 6th day	,,	,,	96 deg.
7.	The 7th day	,,	,,	112 deg.
8.	The 8th day	,,	,,	128 deg.
9.	The 9th day	,,	,,	144 deg.
10.	The 10th day	,,	,,	160 deg.
11.	The 11th day	,,	,,	176 deg.
12.	The 12th day	,,	,,	192 deg.
13.	The 13th day	,,	,,	208 deg.
14.	The 14th day	,,	,,	224 deg.
15.	The 15th day	,,	,,	240 deg.
16.	The 16th day for 224 deg. of advance it retrogrades*			16 deg.
17.	The 17th day for 208 deg.	,,	,,	32 deg.
18.	The 18th day for 192 deg.	,,	,,	48 deg.
19.	The 19th day for 176 deg.	,,	,,	64 deg.
20.	The 20th day for 160 deg.	,,	,,	80 deg.
21.	The 21st day for 144 deg.	,,	,,	96 deg.
22.	The 22nd day for 128 deg.	,,	,,	112 deg.
23.	The 23rd day for 112 deg.	,,	,,	128 deg.
24.	The 24th day for 96 deg.	,,	,,	144 deg.
25.	The 25th day for 80 deg.	,,	,,	30 deg.
26.	The 26th day for 32 deg.	,,	,,	56 deg.
27.	The 27th day for 23 deg.	,,	,,	12 deg.
28.	The 28th day for 15 deg.	,,	,,	26 deg.
29.	The 29th day for $5\frac{1}{20}$ deg.	,,	,,	$4\frac{2}{3}$ deg.
30.	The 30th day the moon is the god Anu.			

The fractions at the end of the tablet are hard to explain, and it is unfortunate that the month is not named during which the observations were made, and that we have no other tablet of a similar kind to compare with it. It will be noticed that here, as everywhere else in Babylonian mathematics, the soss or 60 was the unit, and also that the path of the moon was divided into 240 (60 × 4) degrees. This corresponds with an analogous division of the equator into 240°, *eta* Piscium being 60°, *gamma* Piscium (or rather *alpha* Pegasi) 80°, and so on. An inner circle was drawn within the equatorial and divided into 120 (60 × 2) degrees, a line passing through η Piscium being 30°, and 10° being marked for every 20° of the equator. The ecliptic, "the yoke of the sky" as it was picturesquely called, was divided into 360°, 30° for each sign.* It is curious that no trace is to be found of the 28 *nakshatras* or lunar mansions of Hindu and Chinese astronomy which have been so confidently assigned to a Babylonian origin. Should M. Biot, however, be right in holding that there were primarily but 24 of these, the four additional ones being added by the Chinese sage, Cheu-kung (B.C. 1100), it is possible that they might be connected with the 24 zodiacal stars which, according to Diodorus, were called "judges" by the Babylonians, 12 being north and 12 south.

The problem of calculating solar eclipses by tracing the shadow as projected on a sphere had also presented itself at an early period. Like eclipses of the moon, eclipses of the sun are spoken of as occurring either "according to calculation" or "contrary to calculation." In a report sent in to one of the later kings of Assyria by the State Astronomer, Abil-Istar states that a watch had been kept on the 28th, 29th, and 30th of Sivan, or May, for an eclipse of the sun, which did not, however, take place after all. The shadow, it is clear, must have fallen outside the field of observation. Besides the more ordinary kind of solar eclipses, mention is made of annular eclipses, which, strangely enough, are never alluded to by classical writers. It is interesting to find that observations were made as early as the time of Sargon of Agané on the varying colour of the sun, especially at the beginning of the year on the 1st of Nisan. Thus in one place we are told that the sun on that day was "bright yellow," in another place that it was "discoloured" (or rather "spotted").

Of the planets, only Mercury, Venus, Mars, Jupiter, and Saturn were known, besides the earth. These, however, excited great attention, and their phenomena were carefully studied. The movements of Venus and Mars especially attracted notice. Among the names given to Mars was that of "the vanishing star," in allusion to its recession from the earth, just as Jupiter was frequently called "the planet of the ecliptic," from its neighbourhood to the latter. The title of Mars just alluded to, however, raises the very interesting question whether the Babylonians had observed the phases as well as the movements of Venus and Mars. Now a report, taken from the "Observations of Bel," distinctly states that Venus "rises, and in its orbit duly grows in size," and this, in combination with the name of Mars as "the vanishing star," shows plainly that the phases of the two planets must have been noticed. Such a fact necessitates the existence of some kind of telescope,

* Literally, "becomes obscure."

* The Babylonian symbol for a degree was the star (*).

however rude; and Mr. Layard's discovery of a crystal magnifying lens at Nineveh indicates that such an instrument may have actually been in use.*

The portion of Chaldean astronomy which was concerned with the planets was unnecessarily complicated by the habit of naming them from the fixed stars near which they happened to be at different times of the year, so that the same planet is often spoken of under varying names. Thus *Nibatanu* was properly Altair, but became a very common title of Mars. The number of the fixed stars observed by the Chaldeans was very great, and again suggests the use of something more than the naked eye. The principal stars had individual names, the rest being included in the constellations to which they belonged. In this way the heavens were mapped out long before the idea of a terrestrial atlas had suggested itself. The identification of the Chaldean constellations and fixed stars is of course a work of considerable difficulty, but the modern representatives of several of them have now been determined, and with the help of these and fresh astronomical texts, there is every reason to hope that our knowledge of the celestial globe of the Babylonians will be as complete as it is in the case of the Greeks and Romans.

A. H. SAYCE

COMTE'S PHILOSOPHY

The Positive Philosophy of Auguste Comte, freely translated and condensed. By Harriet Martineau. In Two Volumes, 8vo. Second Edition. (London: Trübner and Co., 1875.)

THE first edition of Miss Martineau's version of the "Positive Philosophy" was published in the autumn of 1853. The considerable space of time which has since elapsed cannot have been due to any defect in the adapter's work. So excellently were the translation and condensation accomplished by Miss Martineau, that Comte substituted her two volumes for his own six volumes, and since Comte's death the work has actually been retranslated into French. It does not give us a great idea of the demand for Comte's works in England, when we find that twenty-two years intervene between the first and second editions. At last, however, the work is re-issued in two handsome volumes, but we are not informed that any alteration at all has been made either in the matter or language of the work, and I have not been able to detect a difference even in a word. The appearance of this new edition nevertheless affords an opportunity for a few remarks upon the value and pretensions of the "Positive Philosophy."

It has been asked "What's in a name?" As regards the positive philosophy, it may be answered that there is a great deal in the name. The name Positive is an admirable *question-begging epithet*. Everything which Comte wished to stamp with his approval, and make a part of his system, he called positive, and a formidable list of new names was invented. We have Positive Philosophy, Positivism, Positivity, Positive Method, Positive Polity, Positive Morality, and even Positive Practices. It would be much more correct to say Comte's Philosophy, Comtism, Comte's Method, Comte's Polity, Comte's Practices, because I believe it is impossible to attribute any invariable meaning to the word Positive, as used by Comte, except that it meant what belonged to his system. Nevertheless, the word was of inestimable value to Comte, because it enabled him to represent all his own views, some being of the most peculiar character, as the natural outcome of the Baconian Philosophy.

We frequently find Comte stating, in the frankest manner, that there was nothing new in the idea of a positive philosophy. Bacon and Descartes (vol. ii., pp. 381, 386, &c.) were the two great legislators of the philosophy. Even the common sense of ordinary thinkers contains all the elements of Positivism, provided that absurd metaphysical and theological ideas do not obscure them. Through Hume, Brown, and a few other philosophers, the pure method of positivism descended to Comte, whose mission it was to develop a complete system of positive thinking. When we attempt to find a clear definition of what the positive method is, it appears to be simply synonymous with the scientific method of induction, resting upon facts. Having thus invested himself with the prestige of whatever is best in the results of modern science, Comte proceeds to deliver at full length his own ideas of the origin and progress of civilisation, the grounds of morality, the best form of government, and the coming system of religious worship. All these ideas, being called positive, are of course the necessary outcome of the pure scientific method.

The following is one of the clearest statements, which I can find, of the nature of the positive method (vol. ii. p. 424):—"The Positive Philosophy is distinguished from the ancient . . . by nothing so much as its rejection of all inquiring into causes, first and final; and its confining research to the invariable relations which constitute natural laws. . . . We have accordingly sanctioned, in the one relation, the now popular maxim of Bacon, that observed facts are the only basis of sound speculation; so that we agree to what I wrote a quarter of a century ago, —that no proposition that is not finally reducible to the enunciation of a fact, particular or general, can offer any real and intelligible meaning. On the other hand, we have repudiated the practice of reducing science to an accumulation of desultory facts, asserting that science, as distinguished from learning, is essentially composed, not of facts, but of laws, so that no separate fact can be incorporated with science till it has been connected with some other, at least by the aid of some justifiable hypothesis." Now this passage not only contains very good sense, but it may be regarded as a most clear statement of what correct scientific method aims at, the ascertainment of general laws. But there is nothing whatever in this to distinguish the positive method from that pursued by all scientific inquirers who have any share of the spirit of Galileo, or Gilbert, or Newton, or Hooke, or Lavoisier, or Laplace, or Faraday. The question really is, then, whether Comte, having properly formulated the method of scientific inquiry, knew how to apply it in regions where he was not led by greater minds. There is no

* A broken tablet I have come across seems to record a transit of Venus across the sun. It is to be hoped that Mr. Smith will before long succeed in bringing to England the remainder of the Kouyunjik Library. At present a tablet is often broken off at its most interesting part, while the corresponding fragment is still lying under the soil on the banks of the Tigris.

doubt that Comte possessed a remarkably extensive and generally accurate knowledge of mathematics, astronomy, and many portions of physics and chemistry, as developed in his day. The first part of his work is therefore comparatively free from objection, and consists to a great extent of an interesting and able review of the progress of physical science.

Incidentally I may remark, that Comte, while continually sheltering himself under Lord Bacon's great name, appears to have known little or nothing of Bacon's works. If there was one thing which Comte abjured, it was the inquiry into causes, whereas Bacon quotes approvingly the old dictum that "truly to know is to know by causes." Every reader of the "Novum Organum" must be aware that Bacon deals not only with causes, but with still vaguer ideas, Forms, Natures, Essences, terms so metaphysical that even the editors of Bacon hardly pretend to make out clearly what they mean. The following is a characteristic extract from the second book of the "Novum Organum" (Aphorism iv.):—"The true form is such that it deduces the given nature from some source of essence which is inherent in things, and is better known to nature, as they say, than Form is. And so this is our judgment and precept respecting a true and perfect axiom for knowledge, that another nature be discovered which shall be convertible with the given nature, and yet be a limitation of a more general nature, like a true genus." It is possible that Bacon knew what he meant, but his own employment of his "true and perfect axiom" was no more happy than I hold Comte's application of his positive method to be.

It is of course impossible to show in a single brief article how crude and unscientific were Comte's results when he applied his method to new fields of research, especially in Sociology. One of his supposed greatest discoveries was the philosophical law of the succession of three states: the primitive theological state, the transient metaphysical, and the final positive state. This is one of those vague and hasty generalisations which have the worst scientific vice of being incapable of precise verification. The theory can be stretched, like india-rubber, to cover any difficulties. If we object that the Hebrews were from the earliest historical times Monotheists, and have so continued to the present day, we are told that they were *prematurely monotheistic*, and are left to imagine that they will ultimately become positivists. What sufficiently condemns Comte's laws of evolution is that they led him away from the doctrines of evolution as now established by Darwin and Spencer, and their followers. Comte was well acquainted with Lamarck's views, which he discusses in Book V. chap. 3, coming to the unfortunate conclusion (vol. i. p. 345) that in every view Lamarck's conception is to be condemned, and "that species remain essentially fixed through all exterior variations compatible with their existence." In the beginning of the fifth chapter of the sixth book, too, we find a passage which entirely cuts Comte off from any share in the sociological doctrines of Spencer. "Gall's cerebral theory," he says (vol. ii. p. 105), "has destroyed for ever the metaphysical fancies of the last century about the origin of man's social tendencies, which are now proved to be inherent in his nature, and not the result of utilitarian considerations." It is highly remarkable that, though the germs of the new philosophy of evolution had been put afloat by the elder Darwin, Lamarck, Malthus, and others, both Comte and his admirer, John Stuart Mill, entirely failed to appreciate their value.

There is no doubt that Comte had very wide and general views as to the possibility of creating great bodies of social science, described by various combinations of the adjective Positive, such as Positive Morality, Positive Polity; but I quite deny that he had any true conception of the proper way of going about the work. It is impossible that he should have, because he altogether abjured and ridiculed that branch of mathematical science, namely, the theory of Probability, by which alone we can approach the scientific investigation of the complex condition of a nation. He says (vol. ii. p. 416): "Mathematicians drop the supposition of natural laws as soon as they encounter phenomena of any considerable degree of complexity, and especially when human action is in any way concerned; as we see by their pretended calculation of chances, through a special application of analysis—an extravagance which is wholly incompatible with true positivity, but from which the vulgar of our algebraists still expect, after a century of wasted labour, the perfecting of some of the most difficult of human studies." It becomes hardly possible to treat Comte's pretensions seriously, when we contemplate this intellectual freak by which he rejects the theory which is becoming more and more the basis of all exact science. The more exact and perfect, in fact, a science becomes, the more complete is the application of the rules derived from the theory of probability. In the computations at Greenwich and other astronomical observatories, they are used in almost every reduction. Nothing is more accurate than a good trigonometrical survey, and yet there is no work to which the theory of chance is more elaborately applied. In proportion as chemistry and physics become exact and methodical sciences, they also resort to the theory of chance, as we see in the researches of Sir B. C. Brodie, or the elaborate labours of Prof. W. H. Miller on standard weights and measures.

As to social science, the Method of Means and the law of divergence from an average, founded on the theory of probability, are simply the alpha and omega of scientific method. We cannot stir a step in any branch of statistical inquiry without drawing an average, and we cannot do this unless we accept the theory which Comte ridiculed. Quetelet is the true founder of exact social science, and his long labours consisted in the unwearied application of the doctrine of chance to vast bodies of statistical facts. In Mr. Francis Galton's works we find the same true method carried out with perfect appreciation of its value.

I might go on to point out, again, that the one branch of social science which most early assumed a partially scientific form, namely, political economy, was that to which Comte entirely refused his *imprimatur*. He never would allow it to be called Positive, though he predicted that in the positive era the world would be governed by bankers. Criticism, however, is disarmed when we consider the vagaries to which the positive method is supposed to have led its great expositor.

W. STANLEY JEVONS

INTERNATIONAL METEOROLOGY

Report on Weather Telegraphy and Storm Warnings to the Meteorological Congress at Vienna, by a Committee appointed at the Leipsig Conference.—Report of the Proceedings of the Conference on Maritime Meteorology, held in London, 1874. (Published by authority of the Meteorological Committee, 1875.)

THE first of these reports is a clear and admirable statement drawn up by Dr. G. Neumayer, of Berlin, secretary to the Committee, of the present position of Meteorology with reference to storm warnings. In this light we recommend it, as well as the appendix which gives the opinions of nearly all our best meteorologists on this important question, for attentive perusal. It is a significant fact, as marking the change of opinion which has taken place since the Dundee meeting of the British Association, that the Committee declare it to be desirable that in all countries in which up to the present time systems of storm warnings have not been organised, steps leading to such an organisation should be taken as soon as possible. What is now required is the further development of the system as regards the principles on which it is based, and its practical application to other public interests than those of commerce and navigation.

The Maritime Conference which met in September 1874 did some goodwork towards securing for meteorology greater exactness and uniformity in observations made at sea—not the least important consideration being the number of countries represented at the Conference, all of which, it may be inferred, will be guided by the decisions arrived at. Of the improvements effected on the Brussels Abstract Log may be noted the recording of the direction and force of the wind as at the time of observation, and not as estimated for a certain number of previous hours, and the recording of the upper and lower clouds in separate columns. The notation of clouds from 0, a clear sky, to 10, an entirely clouded sky, is also an improvement as being in accordance with the procedure now adopted on land. As regards the discussion of ocean statistics, the decision is in every way admirable, viz., that the observations and results be published in such a manner that every foreign institute may be able to incorporate them with its own observations and results; that, to this end, the number of observations, as well as the means deduced from them, be preserved for single degrees square, and that, whatever charts be published, the results for single degrees square be printed in a tabular form.

In the proposed English instructions for keeping the log, we regret to see it stated that for all except wind observations it is sufficient to observe at the four-hourly periods, viz. at 4, 8, 12, A.M. and P.M. A strong recommendation should have been made to make the 10 A.M. and P.M. observations, particularly with the view of arriving at a correct knowledge of the distribution over the ocean of the daily barometric fluctuation which is of so great importance in its connection with atmospheric physics. Since by the hours recommended, no systematic observation will be made from 8 to 12 A.M. and P.M., the two daily maxima of atmospheric pressure will remain wholly unobserved, even approximately.

The box for protecting the thermometers on board, figured at p. 53 of the Report, is of faulty construction—the louvres being single and too wide apart to afford the required protection from the disturbing influences which are so great on board ship. A double-louvred box of the pattern, for instance, of Stevenson's, now so extensively used on land, is indispensable. An arrangement of this sort is the more desirable when it is considered how important it often is in practical navigation to know with exactness the difference between the temperature of the air and that of the sea.

It is with much satisfaction that we notice at pp. 19 and 20, the resolutions passed with reference to the cooperation of the navies of different countries in the working out of the problems of ocean meteorology. Doubtless the time will soon come when the navy will occupy, in practical ocean meteorology, the place occupied in land meteorology by the Central Office in prosecuting instrumental and physical researches; and when it will seriously grapple with the difficult problems of making *real* wind, rain, and hygrometric observations at sea; make hourly observations for determining the constants of temperature, humidity, and pressure over the ocean; and make observations at outlying stations, and observations at 0h. 43m. Greenwich mean time, in connection with the United States Signal Office; as well as collect data on matters more immediately connected with physical geography, such as those with which the *Challenger* has enriched physical science. Towards the bringing about of these desired results, the resolutions of the Conference are well-timed.

OUR BOOK SHELF

Rambles in search of Shells. By J. E. Harting, F.L.S., F.Z.S. (London: John Van Voorst, 1875.)

SAYS the author of this small work, in his introduction: "It has often been a matter of surprise to us that the study of the land and freshwater shells has not more votaries, especially amongst the fair sex. The subject may be easily coupled with botany, being, as it were, nearly associated with it; for, whether we ramble on the downs, in the woodland, or in the marsh, in search of any particular plant, we seldom fail to find in close proximity to it some species or other of mollusca which claims its shelter or support." The large field of entertaining detail—comparatively little trodden, except by the erudite few—which is opened up by a study of shells and their inmates, cannot be better entered than by a perusal of the work before us. Mr. Harting has a happy way of placing the rudiments of a science in a light which goes far to remove the comparative uninterestingness of its bare facts. These latter he intersperses with references to easily appreciated and well-known collateral associations, which retain the attention of the reader, at the same time that nothing is taught but trustworthy and important principles. It is evident that, to the beginner, the classification adopted by systematists is comparatively unintelligible, and often only confusing. That based upon the localities and characteristic soils which the different species inhabit, being at first sight much the more simple, is the one adopted. Accordingly, we find chapters devoted to the shells found on the London Clay, others on chalk soils, &c.; the less common species, from whatever soil, being described in proximity to their better known and nearest allies. Several carefully-drawn coloured plates of the species described greatly facilitate the identification of each. A useful appendix also is a list of the local catalogues of the native land and freshwater mollusca, with the assistance of which the study, commenced in the work itself, can be

extended by the enthusiastic local collector. The number of species described as undoubtedly British is one hundred and twenty, including the slugs, which, "though generally regarded as shell-less, have the shell placed beneath the mantle."

A Manual of the Mollusca. By S. P. Woodward. Third Edition. (London: Lockwood and Co., 1875.)

IN noticing a third edition of the late Mr. S. P Woodward's well-known "Manual of the Mollusca," our object is only to indicate wherein it differs from its predecessors. The body of the work is unaltered; whilst the new editor, Mr. Ralph Tate, in order to bring the work up to the present state of our knowledge, has added an appendix, containing the description of those recent and fossil genera which, either from more recent discovery or oversight, are not to be found in it. This appendix, with its separate index, occupies eighty-five pages, and is illustrated with twenty-seven woodcuts, including drawings of *Clydonites costatus*, *Cochloceras fischeri* (Hauer), *Eucyclus goniatus* (Desl.), *Nucleospira ventricosa* (Hall), &c. Its separate existence we do not object to, on account of the expensive typography of a work of the kind; nevertheless, the outlay involved in an incorporation of the two indexes into a single whole would have been fully made up for by the extra facility of reference afforded, and the diminution in the chance of any additional remarks on previously described genus being overlooked. In the preface to the second edition, which is retained in that under notice, it is remarked that "the chapter on Tunicata has been omitted, since they are more nearly allied to the Polozoa than to the Mollusca proper, and since the Molluscoidan group would have made the work inconveniently bulky." Such being the case, we cannot help asking why the Brachiopoda are not also removed. Is it not because they have shells, whilst the Ascidians are deficient in indestructible parts; not, by the way, that Ascidians are Molluscoidan now-a-days. Additional remarks will be found on the nature of *Belemnites*; that *Crioceras* must merge into *Ancyloceras* is shown to be certain; the genera *Vermetus* and *Siliquaria* are placed in a family by themselves, at the same time that their differences from the mimetic *Serpulidæ* are explained. Several of the families are re-arranged, at the same time that the newly added genera are introduced. The work with the appendix is as accurate a representation of the state of conchology in 1871 as was the first edition on its publication. We put it thus because we can find no difference between this third edition and the second, which has latterly been been bound up with Mr. Tate's appendix in exactly the same form as it appears in the newly produced work.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*]

Oceanic Circulation

I HAVE just read Dr. Carpenter's letter in NATURE (vol. xii. p. 454) in reference to my paper on the *Challenger's* crucial test of the wind and gravitation theories of oceanic circulation, read before the British Association, and am somewhat astonished at the nature of the objections which he advances.

"The doctrine," says Dr. Carpenter, "to which he (Mr. Croll) applied his test, was not mine, but a creation of his own. For his whole argument was based on the assumption that the ocean is in a state of static equilibrium; whereas the theory I advocate is, that the ocean never is and never can be in a state of equilibrium, so long as one part of it is subjected to polar cold and another to equatorial heat, but that it is in a state of constant endeavour to recover the equilibrium which is as constantly being disturbed."

Those who were present at the meeting and heard my paper read, or who have since seen it in the September number of the *Philosophical Magazine*, will no doubt feel surprised that the following paragraph should have escaped Dr. Carpenter's notice:—

"It will not do as an objection to assert that according to the gravitation theory the ocean never attains to a condition of static equilibrium. This is perfectly true, as I have shown on a former occasion;* but then it is the equator that is kept below and the poles above the level of equilibrium; consequently the disturbance of equilibrium between the equatorial and polar columns would actually tend to make the difference of level between the equator and the Atlantic greater than $3\frac{1}{2}$ feet, and not less, as the objection would imply."

If Dr. Carpenter will refer to my examination of the mechanics of the gravitation theory in the *Philosophical Magazine* for October 1871, "Climate and Time," chaps. ix., x., alluded to in the above paragraph, he will find page after page devoted to prove that a constant disturbance both of *level* and of *static equilibrium* is a necessary condition to circulation by gravity. Physicists may differ from me in regard to whether or not the present difference of temperature between the ocean in equatorial and polar regions is sufficient to produce circulation, but I do not expect that anyone familiar with mechanics, *who has been at the trouble to read what I have written on the subject*, will do so materially in regard to the way in which difference of temperature is conceived to produce motion.

It is singular that Dr. Carpenter should not have observed that his objection strengthens my argument instead of weakening it. For if it be true that the equatorial column, though in a state of constant upward motion, never attains to the height required to balance the polar column, then it must follow as a necessary consequence that the rise from the equator to latitude 38° in North Atlantic must be greater than I have estimated it to be; and, therefore, so much the more impossible is it that there can be any surface flow from the equator to the pole due to gravity.

The next objection is as follows:—"The only objection raised by Mr. Croll which has even a show of validity is based on the supposed 'viscosity' of water, which he asserts to be sufficient to prevent the disturbance of thermal equilibrium from exerting the effect which the gravitation theory attributes to it."

What possible connection can "viscosity" have with the crucial test argument? Suppose water to be a perfect fluid and absolutely frictionless: this would not in any way enable it to *flow up-hill*.

The crucial test argument brings the question at issue, in so far as the North Atlantic is concerned, within very narrow limits. The point at issue is now simply this: *Does it follow, or does it not, from the temperature-soundings given in Dr. Carpenter's own section, that the North Atlantic at lat.* 38° *is above the level of the equator?* If he or anyone else will prove that it does not, I shall at once abandon the crucial test argument and acknowledge my mistake; but if they fail to do this, I submit that they ought at least in all fairness to admit that in so far as the North Atlantic is concerned, the gravitation theory is untenable.

The Atlantic column is lengthened by heat no less than eight feet above what it would otherwise be were the water of the uniform temperature of 32° F., whereas the equatorial column is lengthened only four feet six inches. The expansion of the Atlantic column below the level of the bottom of the equatorial not being, of course, taken into account. How then is it possible that the equatorial column can be above the level of the Atlantic column? And if not, let it be explained how a surface-flow from the equator pole-wards, resulting from gravity, is to be obtained.

JAMES CROLL

Edinburgh, Sept. 29

Dehiscence of *Collomia grandiflora*

THE following account of some observations of mine on the dehiscence of *Collomia grandiflora* may possibly prove interesting to some of your botanical readers. I can find no allusion to the singular mode in which the capsules as well as the seeds of this plant become liberated. The fruit is a three-celled capsule, and is almost wholly included within the tube of the calyx. When quite ripe it is of a pale straw colour, and becomes cartilaginous and highly polished, as does also the internal surface of the calyx tube. The latter is ribbed with fifteen prominent lines disposed in threes, each set pertaining respectively to the five sepals, and extending into their free portions. These ridges may possibly help to give direction to the capsule during its exit. Dehiscence

* Phil. Mag., Oct. 1871; "Climate and Time;" chap. ix.

takes place loculicidally, and the three dark-brown seeds, one in each cell, are exposed to view. It is at this stage that the phenomenon in question may be observed. The pressure exerted by the smooth sides of the somewhat obconical capsule against the equally polished surface of the calyx-tube occasions the rupture of the capsule from the base of the calyx, and its more or less rapid expulsion into the air with its three seeds. The latter, which are at this time free within the cells of the capsule, are carried to greater distances on account of the smaller amount of resistance they offer to the air by reason of their shape and weight; the action, in fact, being not altogether unlike that of the discharge of a cartridge and its contents from a rifle. The suddenness of the explosion depends very much on the state of the atmosphere at the time. On a hot day I have observed several instances of spontaneous discharges, whilst a slight touch only was necessary for the explosion of the remaining capsules whose dehiscence had already commenced. Many of the seeds were observed adhering to the upper leaves and calyx-segments, which are thickly covered with glandular hairs of a remarkably viscid nature. Contact with these moist bodies very soon induces the outgrowth of those curious and beautiful spiral hairs for which the seeds of this and a few other plants are remarkable, and thus they become doubly secured by adhesion. I have noticed in some cases when seeds adhere to the flat surface of a viscid leaf, that this outgrowth assumes a definite outline extending all round the seed in the form of a flat membranous expansion, and these, on removal, recall forcibly the appearance of ordinary winged seeds, like those of *Lepigonum marginatum*, for instance. Can this attachment be of any use to the seeds or to the plant itself by feeding on the nitrogenous products of their decomposition? Although I have observed a few of these attached seeds undergoing partial decay, yet, from the nature of their hard horny perisperm, it is not reasonable to suppose that it can take place to any great extent, unless the viscid secretion from the glands is able to render this substance sufficiently soluble for the purpose. If, however, a certain proportion do become sacrificed for the good of the plant, we can understand the object not only of the delicate spiral hairs for ensuring firm attachment, but also that of the explosive process, by means of which a certain number of seeds are conveyed beyond the reach of the viscid surfaces, and falling to the ground, are available for the reproduction of the plant. *Saxifraga tridactylites* might be mentioned as another instance of a viscid plant with the habit of retaining the seeds on its glandular parts; the much larger quantity, however, produced by this latter plant in proportion to what can be required for reproductive purposes would seem to do away with the necessity for any sudden mode of expulsion. Like most plants with sticky glandular hairs, the viscid parts of this Collomia may be seen covered with small insects in various stages of decomposition.

It might be asked, "What advantage can it be for an annual plant to feed on its own seeds, the production of which is the completion and, in a certain sense, the object of its existence?" I would suggest, though with diffidence, the possibility of certain annuals being raised by such means to a higher state of existence as biennials or perennials, in which condition they might or might not require the continued assistance of glandular hairs or other such contrivances. This might explain the occurrence of hairs on certain parts of plants either constantly present or at particular times of their life; such, for instance, as those on the first leaves of the turnip plant, and many other examples could be given, in the case of which we might suppose that the possession of such hairs, or whatever they may represent, have ceased to be required.

There does seem to be some sort of general relation as to the degree of hairiness between annuals, biennials, and perennials, and which often becomes apparent during the development of many plants which in their adult condition are destitute of hairs. On this hypothesis it seems to me conceivable that many of our large glabrous-leaved trees may have originated from hairy or glandular annuals, dependent, perhaps, more or less on aërial nitrogenous food. In any case it is interesting to investigate the true purpose—for such there must be—of the elaborate machinery of traps and spring-guns as displayed in the life of this Collomia. J. F. DUTHIE

Royal Agricultural College, Cirencester

P.S. Since the above was written I have observed the effect of placing a few of the empty expanded capsules in water. In a short time (about half an hour) their valves became completely contiguous, and they presented the same appearance as they did shortly before dehiscence, with the exception of a transparency due to their containing water instead of seeds. This sensitiveness to the action of moisture is clearly a provision for preventing the filamentous outgrowth from the surface of the seeds whilst in the capsule J. F. D.

Lunar Phenomena

I HAVE pleasure in forwarding a brief account of facts relating to two very remarkable protuberances which were observed on the moon's disc in the Gulf of Siam, by Mr. E. C. Davidson, Telegraphic Engineer, and myself.

H.S.M.'s guard-ship *Coronation* (Champon Bay), July 13 (civil time), in lat. 10° 27′ 40″ N. and long. 99° 15′ E., at midnight, the moon bore S.W. by W. magnetic, and its altitude was about 20°, when a prominent projection was seen with the naked eye on the moon's upper limb. The best glasses on board were soon brought to bear upon it, and the enclosed sketches * (with due regard to proportion) were carefully made on the spot.

The protuberance, in colour, was similar to that of the moon.

On July 14, at 8 P.M., the moon was observed perfectly clear, but without a vestige left of the protuberance of the previous night. At this hour, however, a small one was noticed in a different position of the limb.

This also had disappeared before the moon rose on the evening of the 15th inst., when it finally presented its usual unbroken appearance. A. J. LOFTUS

Champon Bay, Gulf of Siam, July 16

The Strength of the Lion and the Tiger

IN NATURE, vol. xii., p. 474, in a review of Dr. Fayrer's book on the tiger, doubts are thrown by the reviewer on the statement that the tiger is stronger than the lion. Dr. Fayrer's statement cannot be contradicted by any person well acquainted with both animals. In my book on "Animal Mechanics," published in 1873, I have proved, p. 392, that the strength of the lion in the fore limbs is only 69·9 per cent. of that of the tiger, and that the strength of his hind limbs is only 65·9 per cent. of that of the tiger.

I may add that five men can easily hold down a lion, while it requires nine men to control a tiger. Martial also states that the tigers always killed the lions in the amphitheatre. The lion is, in truth, a pretentious humbug, and owes his reputation to his imposing mane, and he will run away like a whipped cur, under circumstances in which the tiger will boldly attack and kill.

At p. 482 you state that Dr. Bolau, of Hamburg, is about to publish an account of the anatomy of a gorilla which nearly reached Hamburg alive, and was preserved in spirits. Your readers will be glad to learn that he has been anticipated by Prof. Macalister, of Trinity College, Dublin, who has already published a full account of a similar animal, which nearly reached Liverpool alive some years ago, and was dissected by myself and Dr. Macalister. A comparison of his muscles with those of man, chimpanzee, and hamadryas, will be found in my "Animal Mechanics," p. 404 *et seq.*

SAMUEL HAUGHTON

Trinity College, Dublin, Oct. 1

A Snake in Ireland

THE enclosed letter to the editor of the Irish *Daily Express* may excite speculation as to how the snake got where it was found. The fact is worthy of record, at any rate, that a snake has been caught in Ireland. What would St. Patrick say?

"Sir,—My gardener this morning killed a large snake in the garden here, measuring five feet long by three inches in circumference. It has a black back, with light yellow belly; I do not know what species it belongs to, but have preserved it in spirits. Is it not very rare to find such in Ireland?—Your obedient servant, "FRANCIS WM. GREENE.

"Kilranalagh, Baltinglass, Co. Wicklow, Sept. 11."

I have not seen it, but my correspondent Lady M. has it in her possession, and remarks that its head is very small and its nose pointed; it is quite five feet long, black, and the colour of

* The sketches are not clear enough to be reproduced.

ashes underneath. It appears by a letter from Mr. Greene, "that a gentleman brought two Indian snakes to Ballinrodan, both of which escaped six or seven years ago; one of them was found half eaten by a pig shortly afterwards, and this might be the other, though how it lived through the winters I do not know."

It would be interesting to ascertain whence the snake came and how it found its way to the proscribed island.

London, Sept. 28 J. FAYRER

Origin of the Numerals

IN the novel "David Elginbrod," by George Macdonald, p. 45, is a suggestion of the origin of the forms of the numerals in daily use, very similar to that indicated by Mr. Donnisthorpe in last week's NATURE, p. 476. The disposition of the lines in some of the figures is very ingenious. G. W. WEBSTER

Chester, Oct. 4

IF your correspondent will refer to Leslie's "Philosophy of Arithmetic," p. 103 *et seq.*, he will find that very much is known respecting the origin of the numerals. By referring to p. 107, same work, he will find that the numerals he gave are wonderfully like the Sanskrit.

Newcastle-upon-Tyne, Oct. 4 WM. LYALL

Scalping

MR. CHARLES C. ABBOTT, in NATURE, vol. xii. p. 369, wishes to learn what other men, if any, besides the North American Indians, have the practice of scalping among them. The question is answered in Southall's "Recent Origin of Man," chap. ii. p. 40. "In this connection we may mention that the custom of *scalping* is not peculiar to the American Indians. Herodotus mentions that it was one of the most characteristic practices of the ancient Scythians. But this is not all: it is stated that the practice prevails at this day among the wild tribes of the frontier in the north-eastern district of Bengal. The *Friend of India*, commenting on this statement, adds: 'The Naga tribes use the scalping-knife with a ferocity that is only equalled by the American Indians, and the scalps are carefully preserved as evidences of their prowess and vengeance over their enemies. On the death of a chief, all the scalps taken by him during his warlike career are burned with his remains.'" G. PEYTON

University of Virginia, U.S.A., Sept. 22

OUR ASTRONOMICAL COLUMN

THE DOUBLE STAR Σ 2120.—As mentioned last week, M. Flammarion advocates the binary character of this star, identifying it, as Sir John Herschel had already done, with H. III. 89. Sir W. Herschel's observation runs thus:—

"III. 89. Ad 63am Herculis. In linea per δ et ε ducta.
1782 Nov. 26. Double. About 4 degrees from δ towards ε Herculis, near the 63rd. Very unequal. L. r.; S. r. Distance 11″ 53‴. Position 47° 48′ n. following."

There is a contradiction here; a position "4 degrees from δ towards ε Herculis," which pretty well agrees with that of Σ 2120, would not be near 63 Herculis, which is little more than 1° s.p. δ.

The formula given in NATURE, vol. xii. p. 147, assigns for the position of the small star at Sir W. Herschel's date—

	Angle ...	36° 39′ ...	Distance	10″·72
The observation has	,, ...	42 12 ...	,,	11 ·18

It is by the difference between these positions, which however it may be remarked is not larger than we occasionally meet with on comparing Sir W. Herschel's measures with recent ones, in cases of stars which there is reason to suppose merely optically double, that the binary nature of the object is considered to be proved by M. Flammarion, as it had been by Sir John Herschel in the paper upon his father's measures, which appears in vol. 35 of the "Memoirs of the Royal Astronomical Society." Until that single observation is supported by curvature in the path of the small star subsequent to its nearest approach to the primary, which if this be really a binary system must probably become sensible within a few years from the present time, the suspicion of rectilinear motion of the small star as the cause of the change of position, representing as it fairly does the measures between 1829 and 1873, is not one perhaps that can be legitimately abandoned. The apparent fixity or nearly so of the principal component to which reference was made in our former remarks, is supported by Dr. Engelmann's comparison of the place deduced from meridian observations at Leipsic in 1867, with Struve's position in "Positiones Mediæ," for which the mean date is 1836·1; for secular proper motion he found $\Delta\alpha = +0s{\cdot}192$, $\Delta\delta = +2''{\cdot}40$—very insignificant quantities, and showing that if proper motion, as we have surmised, enters into the question, it is mainly the smaller star that is affected by it. M. Flammarion, relying as stated upon Sir W. Herschel's measure of 1782, concludes: "C'est donc un système orbital très-incliné, et c'est peut-etre celui dont l'aspect ressemble le plus aux systêmes de perspective." We leave it for the measures that may be made during the next few years to decide between these opinions.

THE NEBULA IN THE PLEIADES.—In No. 5 of "Publicazioni del Reale Osservatorio di Brera in Milano," Herr Tempel has laid down the stars in the Pleiades, from the "Durchmusterung," and traced the outline of the nebula near Merope as it appeared to him with a magnifying power of twenty-four on a telescope of four inches aperture. The outline is shown to be elliptical, one extremity of the longer axis, the northern one, at Merope, and the inclination of this axis to the circle of declination about 18°, so that as referred to Merope, the angle of position of the longer axis is 198°; the greatest and least diameters of the ellipse are roughly 35′ and 20′.

M. Wolf, of the Observatory of Paris, observing with the telescope of $0^{m}{\cdot}31$ aperture in March 1874, perceived two nuclei, one almost concentric with Merope, the other and brighter of the two at a distance of about seven seconds, on the same parallel, following. From the month of November 1874 to the end of February 1875 the nebula could not be seen notwithstanding the very favourable atmospheric conditions, and at the same time M. Stéphan was unable to detect it with the telescope of $0^{m}{\cdot}80$. M. Wolf concludes that the nebula is certainly variable, and that its period is pretty short.

Herr Tempel remarks that generally the nebula has been much more readily seen with small telescopes than with large ones, and doubt has been expressed as to any real variability of light; yet it is not easy to understand, except upon this supposition, why the nebula should be visible at certain times in a particular telescope and invisible at others, the circumstances of sky appearing to be about the same in all cases.

This nebula was first remarked by Herr Tempel, at Venice, on the 23rd of October, 1859.

THE SATELLITES OF URANUS AND NEPTUNE.—An elaborate and highly interesting investigation of the elements of these satellites from observations with the 26-inch equatorial of the United States Naval Observatory, Washington, and of the masses of the primaries thereby indicated, has been received from Prof. Newcomb during the past week; it forms an appendix to the Washington Observations for 1873. The most probable value of the mass of Uranus derived from these observations is $\frac{1}{22600}$, with a probable error of 100 in the denominator of the fraction. For Neptune the value of the mass by satellite-observations is $\frac{1}{19380}$; the mass deduced by Prof. Newcomb from the perturbations of Uranus having been $\frac{1}{19700}$: the value resulting from the satellite-observations is preferred. A further account of this important memoir by the eminent American astronomer is reserved for next week.

THE MINOR PLANETS.—M. Leverrier's *Bulletin International* of Sept. 30 mentions the observation of a small planet, on Sept. 21st, by M. Perrotin at Toulouse, 13th mag., which may possibly be new, though at present there is a chance of its identity with No. 77, which is in the same quarter of the sky and has not been observed since 1868, or with No. 137, of which no elements have yet appeared. Its place at 8 P.M. was in R.A. 23h. 16m. 8s., and N.P.D. 95° 12′.

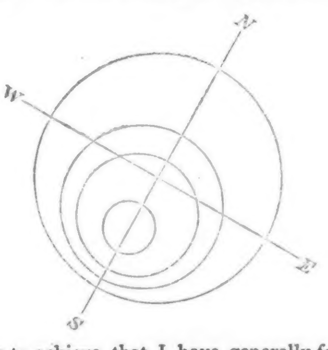
THE TOTAL SOLAR ECLIPSE OF 1878, JULY 29.—The *American Ephemeris* for 1878 is published. The elements of the total eclipse of the sun on July 29, derived from the Lunar Tables of Prof. Peirce, which are adopted for the calculations in that work, are almost identical with those of the *Nautical Almanac,* founded upon the Tables of Hansen, Denver. Colorado appears to be one of the principal places within the limits of the shadow, though some distance from the central line. The sun will be centrally eclipsed on the meridian, according to the *American Ephemeris,* in long. 139° 8′ W., lat. 60° 32′ N.; and according to the *Nautical Almanac,* in long. 139° 10′ W., lat. 60° 27′ N.

MAYER'S METHOD OF OBTAINING THE ISOTHERMALS OF THE SOLAR DISC

THE short notice which I published of my "Discovery of a method of obtaining thermographs of the isothermal lines of the solar disc" was so concisely written that the precautions which are necessary in this new method of research were omitted; but as the republication of my paper in NATURE (vol. xii. p. 301) and in other European journals may induce those engaged in astronomical physics to try the process, I think it proper that I should call attention to some very important experimental conditions to be fulfilled before accurate results can be reached.

1. Special precautions must be taken to prevent currents of air from acting on the film of double iodide.

2. If the image of the sun be formed on the blackened side of the paper, it is absolutely necessary that uniformity should be given to this coating of lamp-black. So difficult is this to achieve that I have generally formed the sun's image directly on the film of iodide. Slight irregularities in this film do not appear to affect the form of the isothermals; but the latter follow irregularities in the smoked surface.



3. The most important, and indeed absolutely essential, condition in these experiments is that the image of the sun shall be formed on a truly *horizontal surface;* for the centre of gravity of any isothermal formed on an *inclined* surface is always *above* the centre of the sun's image and in a vertical plane passing through this centre. Hence all isothermals thus formed are very excentric when referred to the sun's centre. They are also elliptical. The accompanying figure gives isothermals obtained on an inclined surface. *N S* is the solar axis. On obtaining these same isothermals on a horizontal surface they were, as near as could be seen, circular and concentric with the sun's image.

Of the influence of an inclined surface in displacing the isothermals there can be no doubt, and the same action has effected all of the results which have been obtained in the employment of thermopiles in connection with the sun's image received on screens attached to equatorial telescopes. This displacement would mislead an observer, and would cause him to be of the opinion that there existed a decided difference of temperature between the north and south solar poles, and between the portions of the periphery of the sun's image near the poles and near the solar equator. Do not these facts reached by me explain the difference in the results obtained by Secchi and Langley?

The above effects of inclined surfaces appear to be caused by a film of hot air which flows up over these surfaces, and especially on the lower surface of the screen. If the sun's image is received on a film of iodide enclosed between plates of glass or of mica, the excentricity of the isothermals is hardly apparent at first; but after some time it appears, produced by the action of the ascending film on the surface of the glass.

The proper method of research is to use a simple Fahrenheit's heliostat with a good plane mirror, and to throw the solar rays in the direction of the polar axis of the instrument. These rays traverse lenses of from 12 to 30 feet focus, and just before they have converged to form the solar image they are reflected perpendicularly, by another plane mirror, on to the horizontal surface of the iodide.

ALFRED M. MAYER

FAYE ON THE LAWS OF STORMS *

Examination of the Theory of Aspiration.—After a somewhat detailed account of opinions held regarding waterspouts in the prehistoric and Roman epochs, and from the sixteenth century downwards, all agreeing in this, that the water of the sea is sucked up to the clouds by these meteors (Fig. 6), M. Faye inquires, How then could it be doubted that waterspouts, and consequently tornadoes, typhoons, &c. are simply phenomena of aspiration?† Such has been in reality, since the time of Franklin, the point of departure for meteorologists; and hence the prevailing notions regarding hurricanes, that they are centripetal and formed by horizontal currents of air flowing from all quarters towards the centre of aspiration.

Clearly in this case the conclusions have not been drawn with the caution which science demands. To accept, with the eyes shut, the most astounding assertions without examination or verification; to believe, for example, that a waterspout could suck up the water of the sea to a height of 2,000 feet when the most powerful pump could not raise it to the height of forty feet; to admit that insubstantial vapours could form a tube whose sides are capable of resisting the whirling masses of water supposed to ascend through it; to assert that deluges of sea-water are engulphed in the clouds where the clouds cannot retain simple drops of rain, is not in accord with the usage of science, and indeed can only be explained by the dominating power of an old prejudice, which is constantly receiving new life and vigour by the persistent testimony of observers already prepossessed in its favour. There is another reason equally good which accounts for this mode of explaining phenomena. Of all physical inquiries, the most difficult are those which belong to the order of mechanics, which as little admit

* Continued from p. 459.

† It not being considered as disputed that a tornado is nothing but a large waterspout, a typhoon only a large tornado, and that there is no essential difference between a cyclone and a typhoon, M. Faye proceeds to test the theory of centripetal aspiration as regards waterspouts and tornadoes, and conceives that the conclusions thus arrived at will have equal weight when applied to the theory of cyclones.

of sentiment in dealing with them as pure mathematics. In those fields of inquiry where pure mechanics can no longer guide us, the crudest hypotheses take root and grow: witness the wild dreams of the astronomers of the seventeenth century. Now the department of mechanics to which falls the exposition of the gyratory movements of liquids and gases, and on which depend exactly the atmospheric phenomena we speak of, does not yet exist, except as a first and most imperfect draft.

Taken thus at unawares, as it were, and compelled to rely on evidence altogether illusory and suggesting unhesitatingly the idea of aspiration on a vast scale, modern meteorology strives at least to free itself from conflicting impossibilities. Thus, instead of making the waterspout suck up water in its ordinary form, it is assumed that this water is first blown into spray by the conflict of the winds at the base of the waterspout, and then whirled aloft in this form. A curious experiment was even made in 1852 at Washington, for the purpose of showing that this is the case. The following account of it is taken from the "Fourth Meteorological Report to the Senate of the United States," by Prof. Espy:—

"The effect produced by the ascent of a column of air in a narrow space may be thus shown:—If we produce a simple rarefaction of two or three inches of mercury in the upper part of a vertical tube a few feet in length and five inches in diameter, by putting it in connection with the central opening of a machine in full blast, the air will rush into the tube by the lower orifice with a speed proportional to the square root of the diminution of pressure, or about 240 feet per second for an inch of mercury. Then, if a basin filled with water is placed under the opening of the tube and the surface of the water be brought to about $2\frac{1}{2}$ inches from the end of the tube, the water in the basin will be quickly sucked up and ascend the tube, and produce in miniature what takes place in a waterspout. If the tube is glass and of the same dimensions, the water will be seen rising in spray in the form of an inverted cone. This experiment was made in a foundry at Washington in the spring of 1852, in the presence of Prof. Henry and several distinguished members of Congress."

It is singular that none of those present at this experiment remarked the difference there is between a tube of metal or of glass and an almost ideal tube whose bounding surface is only thin insubstantial vapour. The experiment is, however, a proof of the resoluteness with which, in this age even, a belief in the powerful upward suction of waterspouts is entertained.

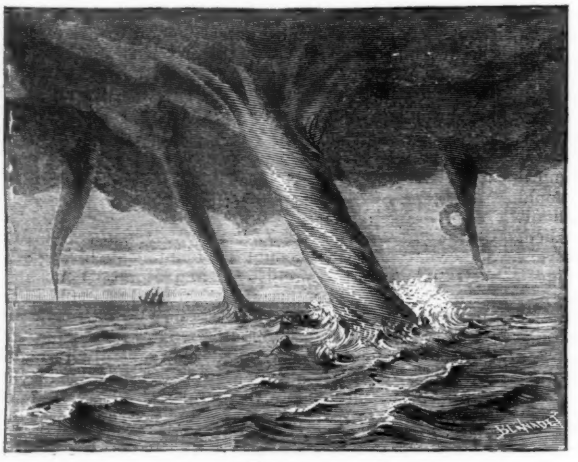


FIG. 6.

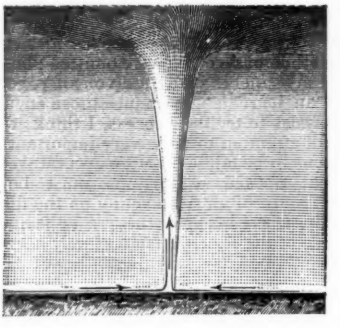


FIG. 7.

In order that an ascending current may take place in the atmosphere for some seconds, it is essential that a mass of a lower stratum of air be heated a little more than the air surrounding it. It thus becomes lighter than the layers above it, and consequently rises. In ascending, however, it expands and cools, and soon all further ascent is arrested at a height where the pressure and temperature equal the pressure and temperature of the ascending mass. It is, moreover, replaced from below by air of a lower temperature from all sides. Up to this point there is little, if any, resemblance to a waterspout; there is, however, already the beginning of a movement of ascension, and by means of some new additional hypotheses the phenomenon is completed by giving to it the essential characteristics of a real waterspout.

Moist air ascends, it is affirmed, more quickly and to a greater height than dry air. Prof. Espy maintains even that it will rise till the limits of the atmosphere be reached in this way:—Moist air in ascending expands and becomes colder; a portion of its aqueous vapour is condensed into mist, and the heat set free in the act of condensation maintains the mass of ascending air constantly at a higher temperature than the stratum of air through which it is ascending. Some physicists consider that these views, thus pushed to exaggeration, are erroneous, but the belief is pretty general, that "the heat due to the condensation" of aqueous vapour is sufficient to raise an ascending column of moist air to a much greater height than an equal column of dry air. Be that as it may, the result would be that when the layers of air resting on the ground are heated by the noonday sun and by radiation, and above all by contact with the ground itself, the equilibrium of the air is disturbed; we should see constantly appearing everywhere a stratum of mist obscuring the rays of the sun. It is useless to point out that this does not represent what takes place. We accept it, however, and proceed.

If we advert to the phenomena of mirage, we find there combined, according to the writers whose theory we are expounding, all the conditions which favour the production of a permanent local indraught of air, and consequently the essential conditions of the waterspout. When the air is perfectly calm and the soil highly heated, the lowest strata of the air are highly heated and thus become specifically lighter than the strata resting over them. But

as this excess of temperature is felt at the same time over a wide area, the lower stratum of air rises bodily, so to speak, over the whole region. Now there is no reason why the air should begin to ascend at one place rather than another in the region where the air is perfectly calm; there will be then between the lowest aërial stratum and the one immediately above it a sort of equilibrium, but an equilibrium so essentially unstable that the slightest accident, such as the striking of a light or the flight of a bird, instantly destroys it. As soon as the charm is broken at some point the lower air will there ascend, and as it is charged with moisture it will continue to rise in an ascending column to the higher regions of the atmosphere. In rising, this air will leave a vacuum below it, towards which will rush the air of a lower stratum. This will in turn follow the first in its ascent, and it is seen that gradually the air of this highly heated lower stratum will flow from all sides with an accelerating speed *towards the pathway opened by the first ascending puff of wind.* As this propagation of the horizontal movement extends wider and wider over the heated stratum, the air which arrives at the place where ascending currents have set in will be of the temperature required to keep up the indraught. Further, the *vis viva* of the air currents about the narrow space where the equilibrium was first disturbed will acquire a force capable of producing, a short distance from the point towards which they all converge, very considerable mechanical effects. Then, if the whirlwind advances on the sea, its surface, lashed on all hands by the converging winds, is thrown into a state of ebullition; the spray is drawn up in an ascending column and whirled aloft, however slight may be the spiral form assumed by the horizontal converging currents. The air which rises so violently in the waterspout will be thrust higher and higher, as we have just seen, by the force constantly called into play by the condensation of the vapour into cloud and rain; at length it reaches the high regions of the atmosphere, where it expands and swells into a dense cloud of enormous dimensions. This, then, is the theory of aspiration.

Before a physicist reasons in this way he ought to be well assured beforehand that the facts are as he supposes; in other words, that waterspouts suck up by a vast upright tube the air and the water of the lower strata. Otherwise he would not fail to remark that if the equilibrium, eminently unstable, which he assumes to be established, comes to be destroyed at any point, it would be quickly destroyed over the whole extent of the lower stratum, the different parts of which would then rise freely, each in its place successively, over the small space required for the re-establishment of the equilibrium of the atmosphere. If, in support of any other theory, a similar mechanical combination were proposed to him, he would reject it without hesitation, and say—in order that such phenomena can take place, in order that the lower air should flow horizontally towards a particular orifice and then rise vertically through this orifice, it would require to be forced to do so by some sort of indefinite but solid boarding placed over the lower stratum of air and pressing on it with all the weight of the atmosphere. If a hole be made in the boarding, the air will pass through it; but even in this case, its ascensional force determined by the slight difference in density between the layers on each side of the boarding will not be great, and the column of air issuing through the orifice will rise to no great height. In no conceivable case can it ever exhibit the terrible and destructive force of waterspouts and whirlwinds, or indeed any distant approach to it, under even the most favourable conditions. Lastly, let it be granted that the facts really are as they are supposed to be, and that the lower stratum of air is on every side in a state of motion towards an orifice of a limited size, where there is no material object to divert it from a horizontal to a vertical course, as in Fig. 7; it is plain that aërial currents could not change their course so abruptly in order to stream through this imaginary orifice; they would instantly enlarge and soon altogether efface from the sky the narrow tube of this meteor to which the

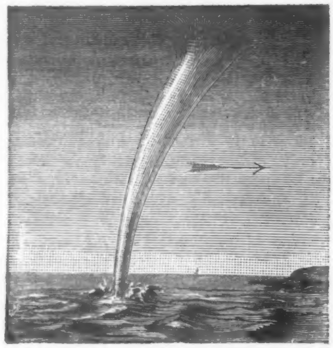


FIG. 8

advocate of aspiration clings because it is the *sine quâ non* of his cherished hypothesis.

But we shall pass over all these impossibilities which prejudice so readily forgets, and consider the consequences which result from this theory—not those which might be drawn to show its utter worthlessness, but those which its own partisans have deduced. It is so easy, from what has been said, to produce a waterspout at will, and everything connected with it—large dense clouds aloft with thunder and torrents of rain—that the idea could not but strike some one. Accordingly, it occurred to several persons in America, where the theory of aspiration has been received as favourably as in France, and the artificial production of a waterspout and a thunderstorm in the United States is gravely related in a letter from Mr. G. Mackay, which letter is published in the "Fourth Meteorological Report to the Senate" (Washington, 1857.) It would be a waste of time to make any further reference to an illusion which puts into man's hands the power of originating waterspouts, tornadoes, and typhoons, simply because it makes the phenomena depend on a state of unstable equilibrium in those layers of the atmosphere which immediately surround us.

Refutation of this Theory.—Let aspiration be established by natural or artificial means at one point in the midst of an absolute calm prevailing in the lower stratum of the atmosphere: there is no reason in such a case why the

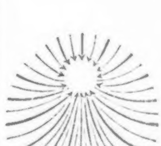


FIG. 9.

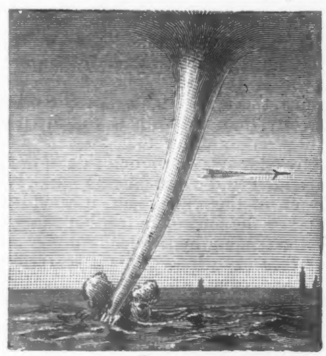


FIG. 10.

centre of aspiration should be displaced, because all is symmetrical and tranquil round this point. Hence it follows:—(1) Waterspouts, tornadoes, typhoons, and cyclones should be stationary. At most the column of

ascending air, when it has reached the elevated regions of the clouds, could not be diverted above by upper currents so as to assume the form represented in Fig. 8; for these upper currents could no more displace the focus of aspiration than they could move a locomotive by deflecting the column of smoke which issues from it. (2) The mechanical effects will be very limited, because the aspiring force being measured by a few millimetres of mercury, were the end of the suction-tube to be plunged into a river or the sea, the water would be raised there a few centimetres. Moreover, at the instant when the extremity of the tube reaches the ground or the water, the air ceases to flow into it and fails any longer to keep the ascending column together, and thus all mechanical action ought to disappear at this moment. Further, it is evident that if the phenomenon has its origin in a perfectly calm stratum of air where not a breath of air is felt, the element of mechanical work, that is to say the force or the motion, fails altogether, or becomes reduced to a feeble ascensional tendency in any stratum of air that may have acquired over the place an abnormal excess of a few degrees of temperature.

Compare now with the facts, these two conclusions drawn from the theory. It would be difficult to find a disagreement more complete. Everyone is aware of the ravages produced by hurricanes, typhoons, tornadoes, and even simple waterspouts and whirlwinds—ravages which imply an enormous development of mechanical force. Then, everyone knows that the peculiarity of all cyclones is to possess a movement of translation, often very rapid, which the theory of centripetal aspiration denies to them. Of all waterspouts hitherto observed, only one instance of a stationary one has been recorded; and even the stationary character in this exceptional case may have been not real but only apparent. As regards tornadoes, all those that traversed the United States since 1811 were propagated onwards with a speed varying from four or five to twenty metres a second. The well-known waterspout of Monville, in France, swept over a league in less than four minutes, or at a rate of about twenty metres per second. At such rates typhoons and cyclones, without exception, also advance; their movement of translation is usually increased as they proceed into higher latitudes, and varies from three to eighteen nautical miles an hour, or from two to ten metres per second.

It has been said by the advocates of the theory of aspiration as applied to hurricanes, that if the converging currents are stronger on one side than on the other, the centre of aspiration, that is the base of the waterspout, will be displaced in the direction indicated by the stronger currents, as shown in Fig. 9. But why this difference of speed, especially over the sea, where there are no inequalities of surface over which the different winds blow? The velocity with which air free to move rushes into a suction-tube is determined by the amount of the suction force; if the movement be impeded on one side of the orifice, the air will enter by the other with a determined velocity, but not with a velocity tripled or quadrupled. Moreover, in order that an excess of velocity of twenty metres per second on one side of the centripetally flowing currents could communicate a like velocity to the onward march of the waterspout, it would be necessary that a wind of the force of a terrible tempest blew in that direction exceeding by a velocity of twenty metres per second the contrary wind. This is scarcely compatible with the absolute calm which ordinarily prevails round waterspouts, tornadoes, and even typhoons.

Fig. 10 represents the appearance of a waterspout whose base is represented as driven forward by a supposed excess of velocity of the inflowing horizontal currents, whilst the top of the ascending column is retarded by the resistance of the air. Now the real figure is that represented in Fig. 11, and it agrees neither with Fig. 9 nor with Fig. 10.

It will be seen on reflection that under all these attempts at explanation there lies a settled conviction which Pliny has aptly expressed in these words: "Quum spissatus humor rigens ipse se sustinet,"—the idea, in fact, which was naïvely reproduced in the experiment at the foundry at Washington, in which it was tacitly assumed that the column of a waterspout or tornado is composed of some rigid material, and that it may be displaced bodily by a force acting on its lower part. In truth, the force which could so act is not to be found. The explanation suggested by Prof. Mohn, that the movement of translation of storms is determined by a difference in the average pressure in the front as compared with the rear of the storm, caused by the condensation of vapour which takes place in front, is insufficient, because we see waterspouts and tornadoes marching onwards, from which not a single drop of rain falls.

No navigator has ever shown that there is in a cyclone the least indication of a decided movement of ascension to which the essential cause of the phenomenon is attributed. Everyone speaks about ascending currents, but no one has seen them, or seems even to have had the idea of verifying their existence in the case of their assumed hurricanes of aspiration. The whole thing is taken for granted, and preconceived notions, whose origin we have traced, have complete control over their thoughts. As regards waterspouts, no manner of doubt is entertained, for the water is seen whirled up their columns skywards. If this were really the case, waterspouts and tornadoes might draw up even to the sky the thousands of trees which they uproot, and a little afterwards furnish the spectacle of a whole forest tumbling from the clouds, it being evident that thousands of trees may be transported as easily to the clouds as thousands of tons of seawater. Eye-witnesses are not wanting to prove that branches of trees have ascended beyond the clouds, they having been seen lying at some distance on the ground, covered with hoar-frost in the middle of summer.

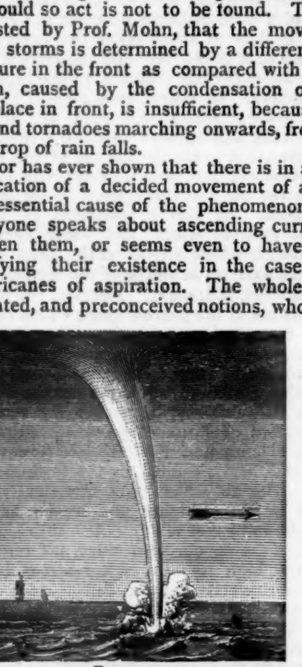


FIG. 11.

What remains then of the theory of hurricanes which is based on centripetal aspiration? It starts from a prejudice, sacrifices at the outset the simplest notions of mechanics, and does not take the trouble to represent a single characteristic trait of the phenomenon. Is it then on this theory we are to rely for the rectification and completion of the laws of storms? Shall we borrow from it, in order to correct the diagrams of Reid and Piddington which are perhaps in some cases too absolutely circular, the hypothesis of centripetal currents suggested by it. Especially shall we sacrifice to it the practical rules of navigation followed during the past thirty years? Unhappily there is some cause for fear, for sailors themselves have long since been prepared by the tales and narrations of the forecastle, for these ideas of aspiration affirmed regarding tornadoes, typhoons, and cyclones. If then they are told that in a particular case one of the

laws of storms has suffered an exception, that the wind has on one occasion not blown perpendicularly to the direction of the centre, they will be tempted to cast aside the rules which have hitherto guided them. This would only be to sacrifice reality to an empty illusion, and science to error.

It is for this reason that we have insisted at some length on a prejudice which might result in consequences so deplorable. But half of our task is still before us. We have yet to point out the true theory of these phenomena, and to show how the sailing rules hitherto adopted are justified by it. In this way will these rules, thus cleared from empiricism, be invested with the authority which they at present stand in need of.

(*To be continued.*)

NOTES

THE following are some of the principal works in the various departments of science and in travel which are announced for publication during the present season. Messrs. Longman and Co. have the following in preparation:—"The Moon and the Condition and Configurations of its Surface," by Edmund Neison, F.R.A.S., illustrated with maps and plates. "An Epitome of the Geology of England and Wales," by Horace B. Woodward, F.G.S., Geologist on the Geological Survey of England and Wales; and a new volume of the "Text-Books of Science," "Telegraphy," by W. H. Preece, C.E., and J. Sivewright, M.A. "Shooting and Climbing in the Tyrol," with an account of the manners and customs of the Tyrolese, by W. A. B. Grohmann, with numerous illustrations from sketches by the author. "The Frosty Caucasus, an account of a walk through part of the Range and of an ascent of Elburz in the summer of 1874, by F. C. Grove, with map, and illustrations engraved on wood by E. Whymper, from photographs taken during the journey. "The Indian Alps and how we crossed them," being a narrative of two years' residence in the Eastern Himalayas, and two months' tour into the interior towards Kinchinjunga and Mount Everest, by a Lady Pioneer. This work will contain a large number of wood engravings and twelve full-page chromolithographs. "A Journey of a Thousand Miles through Egypt and Nubia to the Second Cataract of the Nile," being a personal narrative of four-and-a-half months' life in a Dahabeeyah on the Nile; with some account of the discovery and excavation of a rock-cut chamber or Speos at Aboo-Simbel; descriptions of the river, the ruins, and the desert, the people met, the places visited, the ways and manners of the natives, &c., by Amelia Edwards, author of "Untrodden Peaks and Unfrequented Valleys," &c. The work will also contain ground plans, facsimiles of inscriptions, a map of the Nile from Alexandria to Dongola, and about seventy illustrations engraved on wood from finished drawings executed on the spot by the author.—Messrs. Sampson Low and Co. have nearly ready for publication Mr. John Forrest's "Explorations in Australia." The work will include three different journeys, namely: (1) Expedition in search of Dr. Leichardt and his party; (2) A journey from Perth to Adelaide, around the Great Australian Bight; (3) From Champion Bay across the desert to the Telegraph and to Adelaide. The book will contain illustrations from the author's sketches. Messrs. Longman have also in the press the following:—A work by Dr. Arthur Leared, on "Morocco and the Moors," being an account of travels, with a general description of the country and its people, with illustrations. A new volume on Assyria, by Mr. George Smith, entitled "Assyrian Discoveries," containing the Chaldean accounts of the Creation, the temptation and fall of man, the Deluge, the Tower of Babel and Confusion of Tongues, Nimrod, &c. This book will be profusely illustrated. A translation of Herr Edouard Mohr's "Nach den Victoriafällen des Zambesi" (reviewed in NATURE, vol. xii. p. 231), containing an account of the South African Diamond Fields, &c., is also promised; it will be accompanied by numerous full-page and other woodcut illustrations, several chromolithographs, and a map.—Messrs. Daldy, Isbister, and Co. have in the press a "Geology for Students and General Readers," embodying the most recent theories and discoveries, by A. H. Green, Professor of Geology and Mining in the Yorkshire College of Science. It will be divided into two parts, the first containing the elements of Physical Geology; and the second, the elements of Stratigraphical Geology. Each part will contain upwards of 100 illustrations by the author.—Messrs. Macmillan and Co. have in preparation for the ensuing season, "A Course of Practical Instruction in Elementary Biology," by Prof. Huxley, F.R.S., and H. N. Martin, B.A. "The Modern Telescope," by J. Norman Lockyer, F.R.S.; lectures delivered at the Royal Institution, with additions by G. M. Seabroke, F.R.A.S. This work will be copiously illustrated, and will be uniform with the author's "Solar Physics." Also a work on "Stethometry: Examination of the Chest by a new and more exact method;" with some of its results in physiology and practical medicine, by A. Ransome, M.D. The two following books of travel will also be published in the autumn by Messrs. Macmillan and Co.:—"The Two Expeditions to Western Yunnan, commanded by Major Sladen and Col. Horace Browne," by Dr. Anderson, Director of the Indian Museum, Calcutta, and Professor of Comparative Anatomy in the Medical College, Calcutta, with numerous maps and illustrations. "The Zoology and Geology of Persia," by W. T. Blanford, with narratives of travel by Majors Lovett, St. John, and Evan Smith, and an introduction by Sir Frederick Goldsmid. This work will contain coloured plates and maps, and will be issued in two octavo volumes.—Among Messrs. Smith, Elder, and Co.'s announcements of forthcoming books we notice the following which may be of interest to our readers:—"Science Byways," by Richard A. Procter; and "Notes on the Climate of the Earth, Past and Present," by Capt. R. A. Sergeaunt, Royal Engineers. This last work will be illustrated with diagrams.

THE Yorkshire College of Science at Leeds, which was informally opened a year ago, was formally "inaugurated" yesterday by the Duke of Devonshire and other eminent men. There was a luncheon in the Great Northern Hotel, and a public meeting in the evening, addressed by the Right Hon. Lyon Playfair and others. The first session of this College, it is said, was as successful as could be expected. We have already stated that we cannot regard this institution on its present basis as satisfactory. Except for students whose education up to a certain point has been complete, the curriculum of a science faculty by itself, however complete, may easily do more good than harm. What we want are not separate science colleges, but first-rate secondary schools in which science should find its proper place. When these secondary schools exist, then the students who have passed through them may benefit from a technical school in which no literature is taught—but not till then.

THE inaugural sitting of the International Geodesical Congress took place on the 20th September at the Ministry of Foreign Affairs, Paris, under the presidency of General Hanez, the delegate for Spain. No delegate was present for Great Britain or for the United States; the German Empire was represented by General de Bayer, the Russian Empire by General de Broch, the Austrian Empire by Dr. Oppolzer; Italy, Belgium, Roumania, Switzerland, and the several German States were also represented. M. Charles Jourdain, member of the French Institute, and general secretary of the Minister of Public Instruction, delivered a speech in the name of M. Wallon, who is travelling in the provinces. It was replied by General Hanez and by Ge-

neral de Bayer. M. Faye spoke in the name of the French section, which had invited a number of eminent men of science to take part in the proceedings. A number of reports of the Permanent Section having been read, the assembly adjourned to the following day. On the following evening a number of the delegates visited the Observatory of Paris. It is stated that the longitude of Palermo and Lisbon will be determined electrically with the instruments which have been used for determining the longitudes of Vienna and Algiers.

A PAIR of Sea Lions are shortly expected at the Brighton Aquarium, from the coast of California. They most probably are specimens of Steller's Sea Lion (*Otaria stelleri*), or of Gillies-pie's Sea Lion (*O. japonica*), judging from the locality whence they were obtained. It must be remembered that the name Sea Lion corresponds with the genus scientifically known as *Otaria*, and that there are several species, two of which—*O. jubata* and *O. pusilla*, both from the Falkland Islands—are represented in the collection of the Zoological Society in Regent's Park. Further information with reference to these interesting animals, from some species of which the so-called sealskin of commerce is obtained, will be found in our abstracts of two lectures delivered in the Zoological Gardens by Mr. J. W. Clarke during the early summer of this year (NATURE, vol. xi. p. 514, and vol. xii. p. 8).

THE organisation of the French meteorological regions is progressing satisfactorily. The example was set by Montpellier for the southern Mediterranean region. The northern Mediterranean region has now been centralised at Marseilles, and will very shortly commence operations. A special Meteorological Congress will be held in Poitiers for the western and south-western regions. The date is not quite determined, but a day in the end of October will probably be chosen."

A NEW Physical Observatory is to be erected at Pawlowsk, in connection with the Imperial Russian Physical Observatory at St. Petersburg.

MR. W. B. HEMSLEY has been appointed librarian to the Lindley Library, at the rooms of the Royal Horticultural Society, South Kensington, in the place of Prof. Thiselton Dyer.

THE Astronomical School established at Montsouris under the authority of the French Bureau des Longitudes was opened on Monday morning at eight o'clock by Capt. Mouchez, the director, and Admiral Paris. The pupils are six in number, all of them being lieutenants in the national navy. The period of study is six months. Every two months two pupils will leave and be replaced by two other naval lieutenants. A number of sailors will be attached to the establishment. The students will be taught the practice of celestial photography, spectroscopy, meridian observations, &c.

WE noticed the establishment of a School of Anthropology as being in preparation in Paris some months ago. We are in a position now to give the complete list of professors and the subjects for the course of lectures:—Broca, anatomical anthropology; Dally, ethnological anthropology; De Mortillet, prehistoric anthropology; Hovelaeque, linguistic anthropology; Topinard, general anthropology; Bertillon, statistical and geographical anthropology. MM. Broca, Dally, and Bertillon are connected with the press, and leading members of the Paris Anthropological Society; M. de Mortillet is the Conservator of the Prehistoric Museum at St. Germains.

A MERIDIAN-ROOM, intended for the observations of the French Bureau des Longitudes, was opened last Saturday by M. Dumesnil. The Bureau is now an independent establishment, having an office for meetings of members and computers in a pavilion belonging to the National Institute.

IT is proposed to hold an Electrical Exhibition in Paris in 1877. It will be held in the Palais de l'Industrie, the object being to illustrate all the applications of electricity to the arts, to industry, and to domestic purposes. This project, which was initiated by Count Hallez d'Arros, has been received with general favour both by the scientific and industrial worlds, and the necessary funds have been already guaranteed. An organising committee is being formed, and the provisional offices of the Exhibition have been established at 86, Rue de la Victoire.

THERE has been recently published in Russia a work by MM. Mendeleéf and Kirpetschoff, on the Compressibility of Gases. The authors have been led to several results which ought to attract the attention of physicists; they tend, in fact, to prove that Mariotte's Law does not hold good at low pressures, and that some of the results of Regnault's experiments do not agree with those obtained in other conditions.

THE Swedish Arctic Exhibition arrived at Hammerfest on Sept. 26, in perfect health and condition. They have brought back a rich naturalist collection and several important hydrographic reports. The mouth of the Jenisei river was reached on the 15th of August, and Professors Nordenskjöld, Sundstroem, and Stuxberg took leave of the expedition four days afterwards. They will return to Sweden *viâ* Siberia.

THE following pretty optical experiment is sent us by Prof. F. E. Nipher. Observe a white cloud through a plate of red glass with one eye, and through green glass with the other eye. After some moments transfer both eyes to the red glass, opening and closing each eye alternately. The strengthening of the red colour in the eye, fatigued by its complementary green, is very striking. The explanation of the phenomenon is of course well known, and many modifications of the experiment will readily suggest themselves.

IT is known to many experimenters that pulverised magnetic oxide of iron is to be preferred to iron filings in making magnetic curves. It is easily pulverised to any desired fineness. We do not know why filings are so universally recommended by writers on this subject.

THE Botanical Society of France has been recognised as an establishment of public utility by a presidential decree of Aug. 26. French botany has recently sustained a great loss in the death (at the age of seventy-two years) of M. Boreau, director of the Botanic Garden of Angers. M. Boreau was the author of a "Flora of Central France and of the Basin of the Loire," a work which has reached its third edition. Many papers by him have appeared in the *Memoirs* of the Société Académique de Maine-et-Loire.

AT the International Medical Congress at Brussels, Prof. Marey gave before a large and interested audience a simple, clear, and very complete account of the principal advances in physiology which are due to the introduction of the graphic method into its means of investigation. The application of the methods of mechanics and physics, he believes, has shown what vast horizons are open to the researches of the physiologist, by proving that now we may calculate exactly infinitely small quantities in space and time.

THE August part, just published, of the *Bulletin* of the French Geographical Society contains a very curious and interesting paper by M. E. Cortambert, on "the geographical distribution of celebrated persons in France, or the density of the intellectual forces in various parts of France." It is intended to accompany a map in which, by various tints of colour, it is attempted to indicate the proportion of notable men which have been born in the various departments of the country. M. Cortambert goes

rapidly over the various regions and departments, indicates the relative proportion of notable men belonging to each, and the particular intellectual product in which each has been most fertile. As might be expected, the north, particularly the basin of the Seine, which includes Paris, the great centre of population, is the richest. Seine-et-Oise, l'Aisne, Seine-Inférieure, Calvados, Champagne, are also marked by a deep tint. In the east, Alsace and Lorraine—which in this respect may yet be considered French—Burgundy, especially the Côte d'Or, Doubs, Lyonnais, and French-speaking Switzerland, all stand out prominent. In the south, Isère, Bouches-du-Rhône, Hérault, Haute-Garonne, Gironde, are the most remarkable. The west, as a whole, is but slightly tinted, notable exceptions being Ille-et-Villaine, Charente-Inférieure, and to some extent Maine-et-Loire and Finistère. In general, however, Brittany, whose inhabitants have many other noble qualities, does not show any great eminence from an intellectual point of view. This M. Cortambert is inclined to attribute to the fact that the Bretons are still to a large extent Celtic; and it is noteworthy that the centre of France, where also the same element is still strong, is also comparatively poor in eminent intellectual products. With regard to the particular kind of intellectual product for which each district is noted, M. Cortambert finds that the north is specially fertile in poets, claiming such names as Malherbe, Corneille, Racine, Molière, Boileau, La Fontaine, Voltaire, Beranger, De Musset; while in science it has produced such names as La Place, Élie de Beaumont, Delambre, Ducange; also not a few men eminent as painters, warriors, musicians, historians, and a large proportion of geographers. From the east come many men who have a world-wide fame in the natural, physical, and medical sciences—Buffon, Cuvier, Daubenton, Berthollet, André Ampère, Jussieu, Bichat, Récamier, Saussure, Bonnet, De Candolle, Agassiz, and others; in other departments also, specially in literature and art, this region has been fertile in great names. The south stands out prominent in the region of orators, but has also produced such men as Fermat, Legendre, Arago, Borda, Montesquieu, Montaigne, Tournefort, and Adanson. Brave sailors and celebrated voyagers are the special product of the west. In Brittany and the Centre, philosophy seems to dominate; to the latter belong Pascal and Descartes, and the daring humourist Rabelais. Altogether M. Cortambert's researches in this direction are of special interest, and will be of real value if he connects the results above indicated, as he states he intends to, with the nature of the physical and ethnographical characteristics of the various regions which he has surveyed.

WE read in the Lille papers that the Catholic University of that town has been granted the use of Saint Eugenie Hospital, under certain restrictions.

THE *Geological Magazine* states that Dr. W. Waagen has been appointed to the post of Palæontologist to the Indian Survey rendered vacant by the death of Dr. Stoliczka.

SCIENTIFIC work will soon be resumed in Paris with activity, the Geographical, Biological, Anthropological, and other societies recommencing work within a few days. The Institute is the only French scientific institution which takes no holiday, even for any religious solemnity or national festivity. The regular weekly meetings were only interrupted *once* during the Commune, when civil war was raging in Paris. M. Élie de Beaumont, who was the perpetual secretary, tried to reach the Institute in order to open the sitting, but he was prevented by insurgents refusing to allow him to cross the barricades.

WE have now the final fasciculi of a work, the publication of which has extended over the last five years, the "Nomenclator Botanicus," by Dr. L. Pfeiffer, of Cassel. In two volumes, amounting to over 3,500 pages, are here enumerated all the names and synonyms which have been applied to classes, orders, tribes, families, divisions, genera, and sub-genera of plants, from the time of Linnæus or earlier to the end of the year 1858, with reference to the place of publication. The work will be indispensable to anyone compiling a monograph of a genus or order. It is intended shortly to continue the work down to the most recent times.

THE intended publication is announced, by subscription, of a "Flora of Clackmannan," by Messrs. James R. and T. Drummond. Subscribers' names are to be sent to Messrs. Maclachlan and Stewart, Edinburgh.

THE Report of the Curators of the Botanical Exchange Club (Dr. J. T. Boswell and Mr. J. F. Duthie) for the last two years has just been published. It gives the new localities for scarce plants discovered during that time, and describes in great detail the observations which have been made on new forms or varieties of British plants.

THE *Photographic News*, in speaking of "Photography and the Illustrated Press," gives some examples of the extent to which the latter is now dependent on the photographic art. The *New York Daily Graphic*, besides often executing its pictures from photographs, employs a photo-mechanical process in the production of some of its work. At the office of the *Moniteur Universel*, which is one of the most extensive printing and publishing establishments in France, arrangements are being made for large photo-printing works, as well as for producing coloured pictures by M. Leon Vidal's photo-chromic process. In this country photography is used to aid the artist in sketching to a great extent. One of these days, no doubt, the *News* believes, we shall have our papers illustrated by photographs *pur et simple*, but even now photography has far more to do with the execution of the illustrations in our journals than most people may be aware of.

"WE were witness,' says the *Photographic News*, "the other day of a very pretty application of light made by a gardener. Everybody knows that the ripening and colouring of fruit are due for the most part to light and heat, and that the roses upon an apple are influenced by the manner in which the sun strikes it. On looking at some fine wall-fruit in a Kentish garden, the proprietor called our attention to the manner in which he allowed his peaches to be partially covered by a leaf or two, in places—namely, where he wished them to remain green—and thus heighten by contrast the purple bloom on other portions of the fruit. There were many examples of a leaf being very sharply photographed upon the fruit, and the grower, by exercising a little care during the ripening season, thus enhanced the beauty of his fruit, and also their value, as in the case of a peach it is not only its flavour, but its appearance, which governs the price at Covent Garden."

A CORRESPONDENT writes as follows to the *Derry Sentinel*:—"On Sunday evening last, while going into the country, I observed at Churchill, Glendermott, a bird which at first sight I could not easily class among any known species. On coming closer, however, I found that it was a white swallow. There was no perceptible difference between it and the common swallow, with the exception of its plumage being of the purest white. Other swallows were flying about at the same time, but this *rara avis* shunned their company, and did not seem anxious to join them, as it flitted about by its solitary self, and kept at a respectful distance from the others. As I have never heard of a white swallow having been seen about this part of the country before, I consider it to be a very strange visitor."

PROF. E. MORREN, of Brussels, has been making some experiments with insectivorous plants, with the result that he combats the view that they possess the power of absorbing and assimi-

lating animal matter, as stated by many observers in this and other countries. He says that so far as *Pinguicula longifolia* and *Drosera rotundifolia* are concerned, at least, he believes that the glutinous excretions of their leaves simply hasten decomposition, which is moreover attended by the usual concomitant phenomena. In very early stages he found monads, bacteria, the mycelium of various fungi, and other conditions of putrefaction. So far as the action of the mucus on the entrapped insects and on coagulated albumen is concerned, he affirms that it is similar to that of pure water, sugar-water, and the honey-secretions taken from the flowers of *Aechmea nudiflora*. Nevertheless he admits having seen all the admirable contrivances for catching and retaining insects.

MR. G. M. DAWSON, F.G.S., has just issued a report to the Canadian Government, on the geology and resources of the region in the forty-ninth parallel, between the Lake of the Woods, S.E. of Lake Winnipeg, and the Rocky Mountains; in other words, of the western portion of the boundary of British America. Much of the country traversed had been previously quite unknown, geographically as well as geologically, which fact adds greatly to the importance of the report, the bulk of which is devoted to the account of the Cretaceous and Tertiary strata of the plains between the Rocky Mountains, as they are constituted at the boundary, and the Lake of the Woods. The Survey of the United States Government to the south of the above-mentioned region, when taken in conjunction with that under notice, forms a vast addition to geologic knowledge. Among the most important results arrived at is the discovery of beds which seem to gap over the apparently considerable interval between the Cretaceous and lower Tertiary periods.

THE following interesting statistics on the libraries of Europe are taken from M. Block's recently published "Statistique de la France comparée avec les divers pays de l'Europe":—Paris has six great libraries belonging to the State and open to the public. Outside Paris there are in France 338 libraries which possess more than 3½ million volumes; of this number 41 are open in the evening. Great Britain possesses 1,771,493 volumes, or six vols. to each 100 persons of the population (this must surely refer solely to the British Museum library). Italy has 11·7 volumes per 100 inhabitants. In France there are 4,389,000 volumes, or 11·7 per 100 persons; in Austria, 2,488,000 vols. or 6·9 per 100; in Russia, 852,000 vols., or 1·3 per 100; in Belgium, 509,100 vols., or 10·4 per 100. Of all countries, France possesses the greatest number of volumes, and Paris alone has one-third of them in its libraries. Since 1865 students' libraries have been formed over nearly the whole of France. Since that year these libraries have increased from 4,833, containing 180,854 volumes, to (in 1870–1) 13,638, containing 1,158,742 volumes.

THE additions to the Zoological Society's Gardens during the past week include four Tigers (*Felis tigris*) from India, presented by H.E. the Governor-General of India; an Ocelot (*Felis pardalis*) from South America, presented by Mr. H. Kirtley; a Golden Agouti (*Dasyprocta aguti*) from South America, presented by Mr. Henry T. Balfour; a Cuvier's Toucan (*Ramphastos cuvieri*) from Upper Amazons, presented by Mr. A. Blumenthal; a Chilian Sea Eagle (*Geranoaëtus aguia*) from Paraguay, presented by Mr. E. Nelson; two Red and Yellow Maccaws (*Ara chloroptera*) from South America, presented by the Misses Rix; three Tigers (*Felis tigris*), a Leopard (*Felis pardus*), a Caracal (*Felis caracal*), two Musanga Paradoxures (*Paradoxurus musanga*) from India, a Black Lemur (*Lemur macaco*) from Madagascar, a Crab-eating Opossum (*Didelphys cancrivora*) from Central America, two Mexican Deer (*Cervus mexicanus*), deposited; a Great-billed Parrakeet (*Tanygnathus megalorhynchus*) from Gilolo, received in exchange; an American Darter (*Plotus anhinga*) from South America, purchased.

SOME LECTURE NOTES ON METEORITES*

II.

WE may next turn our attention to the nature of the substances which fall on these occasions, and in the first place it may be briefly stated that they are of three kinds: first, masses of iron, alloyed with nickel, termed aërosiderites, or briefly siderites; secondly, stony meteorites (aërolites), which consist of silicates somewhat analogous to terrestrial rocks, but having nickeliferous iron disseminated in small granules throughout them: and finally, there is a sort of meteorite which is intermediate between these iron and stone masses, consisting of a sponge-like mass of the iron, containing in its hollows stony matter similar to that of the aërolites. These are what are termed siderolites (or meso-siderites). These different kinds of meteorites—namely, siderites, siderolites, and aërolites—then, comprehend all the forms of matter, as at present known, which fall to the earth from the regions external to its atmosphere.

Of these different kinds of meteorites, national as well as private collections have been formed in most countries in Europe. The most celebrated and historical collection of them is that at Vienna, formed by the gradual and generally contemporary acquisition of specimens of the meteorites as they have fallen or been found from time to time, from the early years of this century, and descriptions of them have been given by very eminent Viennese mineralogists. Then we have in the British Museum a not less complete collection, numbering now about 294 different meteorites. Next to these in completeness is the collection at Berlin, founded on that formed by Chladni.

The importance of the study of such collections of meteorites becomes evident, if we consider a remark of Humboldt's, in the latter part of his "Cosmos," to the effect that there are only two avenues to our knowledge of the universe outside of us, one being light, by the agency of which the motions of the heavenly bodies are revealed to us, while the other consists in the masses of matter that come to our earth from that outer universe; and that these are the only means by which we are able to take any cognisance of what is going on in the boundless regions of space.

Since Humboldt's time, indeed, light has become a totally different instrument in our hands to what it was. No longer are the heavens for us without speech or language, for light is indeed the language of the universe, though man has only yesterday begun to interpret the voices whereby one star calleth to another star.

Our interpreter is the prism, that most subtle and sensitive implement for probing the character of the most distant matter provided only it be luminous. In Humboldt's time light merely enabled us to record and calculate the mute motions of the orbs around us. Now not only are we able so to tell their motions, but we may feel new truths "trembling along that far-reaching line" which connects our eye with a star, and take cognisance of the physical conditions and chemical composition of the matter in active change upon the surface of that star. And this altogether new source of knowledge throws an entirely new interest around the question of the origin or sources of meteoric matter. Let us then next inquire of the meteorites themselves what they have to tell us in elucidation of these questions.

The first aspect of a meteorite is that of a fragment. One cannot look at it without saying so. But as to the question whether it came as a fragment into our atmosphere, or whether it became a fragment after it had entered it, we can at least say that its present fragmentary form is mainly due to the action of that atmosphere itself. Still, it is eminently probable, from other grounds, that meteorites encounter our earth, and probably our system, in the guise of fragments, or rather of angular and unshaped masses—chips, as it were, thrown off in the great workshop; matter flung out into space, not yet used up in the making of the worlds. It will be well first to consider what an examination of their physical characters and general internal structure will reveal to us. For the incrustation and pitted surface of aërolites already described an explanation was sought on the hypothesis of external fusion arising from the sudden development of enormous heat on the surface of a mass internally brittle and contracted, owing to its very low temperature. And among the more purely mechanical characteristics, we must not pass over the general want of compactness in meteorites. Thus, though a meteorite generally seems very compact, if it be suspended in chloride of mercury to dissolve the iron without affecting, or with only slight effect on, the other minerals in it, you

* Continued from p. 487.

will dissolve meteoric iron out of it; but the remainder of the mass will, after this treatment, in most cases, crumble into a granular powder, showing that the cohesion of the mass is not like that of an ordinary terrestrial rock. Some aërolites, again, will even crumble in the fingers without previous treatment.

The rocks to which they bear the nearest resemblance, in respect of their mechanical structure, among the products of our volcanoes, are some volcanic bombs, and, as regards several of the aërolites, certain kinds of volcanic tufa.

Now, in examining these bodies more closely, the first thing that calls for attention is that they are composed entirely, or almost entirely, of crystalline substances; and that matter thus coming from regions beyond our world crystallises in the same way, and is obedient to the same law, as matter which crystallises on the globe.

Sections of meteorites cut thin and ground down to transparent slices, when examined by means of polarised light, are seen to be crystallised throughout; the crystalline character of the substances being evidenced by the interference tints which colour the different crystals of which the sections are made up. Another characteristic of many meteorites, in which they differ from ordinary terrestrial rocks, is what has been termed by Gustav Rose their chondritic structure. The minerals in these are found to be more or less aggregated in little spherules, which are distributed in different degrees of abundance in different meteorites through the ground-mass of the stone.

Sections of chondritic meteorites show them to consist in some cases almost entirely of spherules. Such is the case with the Parnallee aërolite, of which the most varied groups of spherules may be seen assembled in a single section. Some of these spherules are encased, as it were, in minute shells of metallic (nickeliferous) iron, or of such iron mingled with a kind of pyrites peculiar to meteorites, an iron sulphide termed troilite, constituted by an equivalent of sulphur combined with one equivalent of iron. Minute granules of troilite and iron, without any definite form, are so seen to be disseminated among the grains of the interspherular ground-mass of the meteorite.

A closer inspection of the spherules further reveals in many cases the presence of *intra*spherular iron. In some spherules the meteoric silicates may be seen, radiating from a point, but while the spherule is enclosed in a mixed outer mass of silicates, iron and troilite in little black specks are seen scattered all through it, presenting the appearance of having been spurted, as it were, from a point, the larger particles to the greater distance: and these specks consist in part of nickeliferous iron, while some are meteoric pyrites (troilite).

In connection with the subject of these spherules, which form so characteristic a feature of many stony meteorites, it should be mentioned that occasionally some of the spherules are seen to be broken in half and the halves separated from each other to some small distance, a fact of considerable significance, though not easy of interpretation in connection with the history of the meteorite and the more or less violent crises it must have passed through at successive periods in that history.

Evidence of another kind of historical succession in the events and influences through which a meteorite may have passed is afforded by the not rare peculiarity of a sort of vein, like a mineral vein, running through the meteorite. In fact, just as in a mine one may meet with a fissure that, once dividing the "country," but subsequently filled by rocky matter, cuts across the course of a mineral vein which itself was originally formed in a similar way; and just as such a cross fissure thus intersecting with the original metalliferous vein often gives us evidence of a *heave*, i.e. that one side of the new fissure has slid upwards or downwards along the other, so an exactly similar thing is met with in meteorites, and is admirably seen in the microscopic sections of them.

Such a fissure will sometimes divide several spherules lying on its track, the two sides of the fissure having slid, the one along the other. The corresponding halves of the spherules are in such cases separated to some distance along the fissure, and this is itself filled with the vein of meteoric iron or troilite, in some cases with a black fused substance, like the crust of a meteorite.

In passing next to the consideration of the chemistry of meteorites, one of the first inquiries that suggests itself is whether and to what extent the elementary composition of these cosmical rock-fragments accords with that of our own world, or with the revelations which the prism has afforded us regarding the constitution of the matter in energetic action on the surface of our sun, or of those far distant suns, the stars; or, again, in those still uninterpreted assemblages of luminous matter that we call the nebulæ. Now, the elements that have been already recognised by analysis as existing in meteorites form a list that comprises one-third of all the elements known to our chemistry; and these, the more abundant elements on our world. They are—

Hydrogen	*Chromium*	Arsenic
Lithium	*Manganese*	Vanadium ?
Sodium	*Iron*	Phosphorus
Potassium	*Nickel*	Sulphur
Magnesium	*Cobalt*	Oxygen
Calcium	*Copper*	Silicon
Aluminium	Tin	Carbon
Titanium	Antimony	Chlorine

Now, of these elements, those in italics have also been found by the spectroscope to be constituents of the solar surface, together with zinc, strontium, and cadmium, which metals have not yet been met with in meteorites.

The number of elements recognised as existing in activity on the solar orb will undoubtedly be largely increased with the progress of the combined study of the solar spectrum and of the conditions under which the several lines belonging to the different elements are developed. It is by study of this kind that Mr. Lockyer has detected potassium in the sun. The fact that at the present time all the elements detected in the sun excepting three are met with in meteorites, while on the other hand the meteorites contain five metals not as yet found in the sun, at the same time that the six metalloids found in them are so strangely all apparently absent from the surface of our great luminary, might seem to place difficulties in the way of our recognising a general unity of elementary composition in the matter that composes the various orbs and wandering masses that pervade our universe.

But it is clear, on the other hand, that it is too early as yet to look on these results as establishing even probable exceptions to such a unity.

That carbon, sulphur, potassium, and phosphorus, elements so frequently met with in meteorites and on our globe, should, with nitrogen, be absent or have escaped detection among the elements involved in the active operations on the surface of the sun, is certainly not a little surprising. Nor is the failure of the prism to detect the lines due to oxygen and silicon among those presented by the solar photosphere to be accounted for by assuming the persistency of particular silicates in resisting decomposition or vaporisation even in a solar temperature, for Von Rath has shown that silicates such as augite and leucite are actually deposited by a process of sublimation even at the comparatively low temperatures of our volcanoes. Yet it is difficult to believe that the last-mentioned elements can be absent from the great central body of our system, whether we reason from analogy, from their great importance in the composition of our earth, or from the more than probability that these elements must have been contributed to a large amount to the material of the sun by meteoric matter falling into his surface.

Mr. Lockyer has indeed grasped this difficulty with a bold hand, and has not hesitated to declare as a probable explanation of the results obtained from the spectra of the reversing layer and chromosphere of the sun, that the elements exist there not in a molecular but in an atomic condition; and he further assumes that the metalloids exist in a more simple elementary condition than that in which we know them; their terrestrial existence being assumed to be that of compounds which have yet to be resolved into their constituents by our chemistry, though under the fierce chemistry of the sun it is only as thus resolved that they exist on his surface. It is startling for the chemist to be thus called upon to believe that enormous temperatures are endowed with a dissociating power, capable of not merely severing the bonds of ordinary chemical combination, but further of forcing into a condition of ultimate atomic disintegration composite molecules where these are the form under which the chemist has learnt to recognise the ordinary condition of even the isolated elements. Certainly the concordance of the heights to which the different gaseous elements rise in the reversing layer with the weights of the atoms of those elements as represented by their equivalents in the older chemistry, would lend something more than a justification to the even bolder hypothesis that recognises in the metalloids (such as silicon, sulphur, and oxygen, as they exist in our world) compounds of other and to our chemistry unknown elements, were we able to assert that the gaseous molecules of the metals in question, other than hydrogen, potassium, and sodium, must necessarily, like those of these elements, be double. It would be, in any case, a

splendid result of solar physics to establish the nature of the gaseous molecules of so many elements that have as yet defied the experimental methods of our terrestrial laboratories. The banded character of the spectra of so many of these metalloids has lent a really important argument to Mr. Lockyer in his bold speculation as to their compound nature, in consequence of its parallelism with the case of compound gases, and his hypothesis has the merit of giving thus an explanation of the apparent absence of elements that every argument would lead us to look for, founded on a principle as ingenious as it is bold in its application.

The recognition by Mr. Huggins in the spectra of the stars of the lines belonging to hydrogen, sodium, magnesium, calcium, and iron, and of carbon compounds in comets and nebulæ, tends strongly to confirm the probability of a general identity in the chemical nature of the matter which pervades our universe; and further shows that the results of these investigations present no obstacle to our drawing any conclusion to which the logic of facts might otherwise guide us as to meteorolitic matter having been in its origin foreign to the solar system. Observations by v. Konkoly of the magnesium, sodium, and possibly also iron lines in the August meteoric swarm, like those by Alexander Herschel of the sodium line in those same St. Lawrence meteors, are of value as extending the coincidence in the elementary constitution of the sun, the stars, and meteorites, to those minuter forms of meteoric matter which, by their dispersion in the atmosphere, have hitherto been unattainable for the purposes of investigation.

In passing from the merely elementary components of meteorites to the chemical forms—that is to say, to the minerals in which these elements are grouped in them, we find ourselves in the presence of aggregates of crystallised minerals that at once remind us of our terrestrial rocks. At a first aspect they might easily be taken for rocks formed under conditions not very different from those of our globe. A closer inspection, however, brings out distinctive characters in these that evidence a very different set of conditions as having prevailed in the formation of the meteoric and the terrestrial rocks. Without going into minute mineralogical variations, and needlessly multiplying names, we may tabulate in a very short list the constituent minerals of the different sorts of meteorites. Several of these minerals are nearly identical in composition and crystallographic character with corresponding minerals met with in terrestrial rocks; others again are unknown, while some of them could hardly exist permanently as terrestrial minerals; and two present the composition of minerals familiar to us in our own rocks, but crystallographically distinct from these as belonging to different types of symmetry or "systems" from theirs.

In the Elementary Condition.

Iron with Nickel, traces of Cobalt and Copper, in some and probably in all cases with Hydrogen, Carbonic oxide, or other gases occluded in the metal.

Carbon (graphitic and plumbaginous).

Sulphur.

Compounds.

Ferrous Sulphide (*Troilite*)	FeS
Magnetic Pyrites	Fe_7S_8
Magnesium Sulphide ?	MgS
Calcium Sulphide (*Oldhamite*)	Ca(Mg)S
A Titanium—Calcium Sulphide (*Osbornite*)	?
Magnetite	Fe_3O_4
Chromite	$(FeCr)_3O_4$
Silica (orthorhombic as *Asmanite*)	SiO_2
,, (hexagonal as Quartz) ? }	
Tin Oxide }	SnO_2
Silicates, viz.:—	
Olivine varieties	$\left(Mg_{\frac{n}{m}}Fe_{\frac{m-n}{m}}\right)_2SiO_4$
Enstatite	$MgSiO_3$
Bronzite varieties	$\left(Mg_{\frac{n}{m}}Fe_{\frac{m-n}{m}}\right)SiO_3$
Augite varieties	$\left(Mg_{\frac{n}{m}}Ca_{\frac{m-n}{m}}\right)SiO_3$
,, varieties containing corresponding ferrous silicate.	
Anorthite	$CaAl_2SiO_8$
Labradorite ?	
,, in tesseral forms (Tschermak's Maskelynite).	
Schreibersite varieties (phosphides of iron and nickel).	
Hydrocarbons (not yet sufficiently investigated).	

The names printed in italics are thus new to our mineralogy. The mineral to which I originally gave the name of Oldhamite is in all probability a mixture of two minerals—a Calcium Sulphide (which would be the pure Oldhamite) and a Magnesium Sulphide; and it is probable that they are not uncommon, though sparsely scattered, ingredients in freshly fallen meteorites, which, however, the action of a damp atmosphere rapidly decomposes into calcium sulphate or carbonate, and free sulphur, all which minerals occur in minute quantities occasionally in meteorites after they have been exposed to the weather.

Until the year 1867 the mineralogical department at the British Museum was without a laboratory, and chemical analyses could not be performed. I accordingly had recourse in 1861 to microscopic investigation as my only means of attacking the mineralogical problems presented by meteoric rocks. By the use of polarised light, of which the position of the plane of polarisation was accurately determined, it was possible, by the aid of an eyepiece goniometer and also of a revolving stage, to determine with some precision the directions of the principal sections in any of the minute sections of crystals which a fragment of a meteorite worked down to a thin transparent slice might present. Where such crystal sections happened to be approximately parallel to a zone plane, and the traces of the faces belonging to the zone could be seen with sufficient sharpness, or where cleavage planes occurred parallel or at recognisable inclinations to faces of the zone, important decisions could be arrived at by aid of polarised light. And this method is now becoming one of great importance to petrologists.

It was thus that I was enabled to anticipate with much confidence the orthorhombic character of one and the clinorhombic character of another ingredient (the enstatite and augite) in the Busti meteorite, and determine the cubic character of the oldhamite in that meteorite in 1862; and to be the first to announce the more than probability of enstatite (including of course, as the term then did, bronzite) being an important ingredient in meteorites; in the case of the Nellore meteorite in June 1863 and of that of Kaee in August 1864; a view confirmed afterwards (in November 1864) by Dr. Lawrence Smith on his repeating his analysis of the meteorite of Bishopville. Of the meteorites of Busti and of Manegaum, before they were cut, only minute fragments were at my disposal; and though in naming and first describing oldhamite in 1862, I had spoken of it as having all the appearance of being a "calcium galena," a small amount of probably sulphur and gypsum that separated in the watch-glass in which I made a qualitative investigation of it constrained me to say that I believed it to contain an excess of sulphur beyond that in the neutral sulphide.

Of the Manegaum meteorite also I employed only a minute fragment for investigation, and I attributed the bronzite of that meteorite to olivine, the section of the crystal examined not being really paralled to a zone-plane, and was confirmed in this error by finding the powdered bronzite not to be insoluble in acids. The addition of a laboratory to the department in 1867 enabled the long-desired analysis of the minerals I had separated to be made; and Dr. W. Flight being at my request appointed chemical assistant, I was able, with the help of his analytical skill, to complete the account of the minerals the presence of which in the meteorites in question had been determined so many years before.

The separated sulphur in the oldhamite proved, when a sufficient amount was taken for investigation, to be due to a superficial decomposition ot the mineral, while bronzite was shown to be distinctly soluble in acid. The methods I adopted for the investigation of meteorites have since been employed by other observers, as well in the mode of using the directions of the principal sections of crystal-sections in the microscopic examination of terrestrial rocks as in the mode of attacking a meteorite by separating and isolating by toilsome microscopic selection its ingredient minerals; the plan by which the silicates in the Breitenbach siderolite and also those in fresh amounts from the Busti aërolite had been separated with a view to analysis in 1864 and 1865. Viktor von Lang, to whose assistance and to whose friendship I owe two or three of the most valued years of my life, while he was my colleagne, measured, and some time afterwards published the account of the crystals of bronzite in the Breitenbach meteorite; the first occasion on which the crystallography of that mineral had been made out, only the system and approximate prism angle of the terrestrial bronzite and enstatite being previously known through the optical researches of Des Cloizeaux.

The form of asmanite, the orthorhombic variety of silica, occur-

ring in the same meteorite, offered a difficult problem which I had taken in hand. One little crystal, however, carrying a portion of a zone with four consecutive faces, picked out in 1867, furnished the final key to its crystallography.

N. S. MASKELYNE

(*To be continued.*)

INSTINCT AND ACQUISITION.*

SO great was the influence of that school of psychology which maintained that we and all other animals had to acquire in the course of our individual lives all the knowledge and skill necessary for our preservation, that many of the very greatest authorities in science refused to believe in those instructive performances of young animals about which the less learned multitude have never had any doubt. For example, Helmholtz, than whom there is not, perhaps, any higher scientific authority, says: "The young chicken very soon pecks at grains of corn, but it pecked while it was still in the shell, and when it hears the hen peck, it pecks again, at first seemingly at random. Then, when it has by chance hit upon a grain, it may, no doubt, learn to notice the field of vision which is at the moment presented to it."

At the meeting of this Association in 1872, I gave a pretty full account of the behaviour of the chicken after its escape from the shell. The facts observed were conclusive against the individual-experience psychology. And they have, as far as I am aware, been received by scientific men without question. I would now add that not only does the chick not require to learn to peck at, to seize, and to swallow small specks of food, but that it is not a fact, as asserted, and generally supposed, that it pecks while still in the shell. The actual mode of self-delivery is just the reverse of pecking. Instead of striking forward and downward (a movement impossible on the part of a bird packed in a shell with its head under its wing), it breaks its way out by vigorously jerking its head upward, while it turns round within the shell, which is cut in two—chipped right round in a perfect circle some distance from the great end.

Though the instincts of animals appear and disappear in such seasonable correspondence with their own wants and the wants of their offspring as to be a standing subject of wonder, they have by no means the fixed and unalterable character by which some would distinguish them from the higher faculties of the human race. They vary in the individuals as does their physical structure. Animals can learn what they did not know by instinct and forget the instinctive knowledge which they never learned, while their instincts will often accommodate themselves to considerable changes in the order of external events. Everybody knows it to be a common practice to hatch ducks' eggs under the common hen, though in such cases the hen has to sit a week longer than on her own eggs. I tried an experiment to ascertain how far the time of sitting could be interfered with in the opposite direction. Two hens became broody on the same day, and I set them on dummies. On the third day I put two chicks a day old to one of the hens. She pecked at them once or twice; seemed rather fidgety, then took to them, called them to her and entered on all the cares of a mother. The other hen was similarly tried, but with a very different result. She pecked at the chickens viciously, and both that day and the next stubbornly refused to have anything to do with them.

The pig is an animal that has its wits about it quite as soon after birth as the chicken. I therefore selected it as a subject of observation. The following are some of my observations:—That vigorous young pigs get up and search for the teat at once, or within one minute after their entrance into the world. That if removed several feet from their mother, when aged only a few minutes, they soon find their way back to her, guided apparently by the grunting she makes in answer to their squeaking. In the case I observed the old sow rose in less than an hour and a half after pigging, and went out to eat; the pigs ran about, tried to eat various matters, followed their mother out, and sucked while she stood eating. One pig I put in a bag the moment it was born and kept it in the dark until it was seven hours old, when I placed it outside the sty, a distance of ten feet from where the sow lay concealed inside the house. The pig soon recognised the low grunting of its mother, went along outside the sty struggling to get under or over the lower bar. At the end of five minutes it succeeded in forcing itself through under the bar at one of the few places where that was possible. No sooner in than it went without a pause into the pig-house to its mother, and was at once like the others in its behaviour. Two little pigs I blindfolded at their birth. One of them I placed with its mother at once: it soon found the teat and began to suck. Six hours later I placed the other a little distance from the sow; it reached her in half a minute, after going about rather vaguely; in half a minute more it found the teat. Next day I found that one of the two left with the mother, blindfolded, had got the blinders off; the other was quite blind, walked about freely, knocking against things. In the afternoon I uncovered its eyes, and it went round and round as if it had had sight, and had suddenly lost it. In ten minutes it was scarcely distinguishable from one that had had sight all along. When placed on a chair it knew the height to require considering, went down on its knees and leapt down. When its eyes had been unveiled twenty minutes I placed it and another twenty feet from the sty. The two reached the mother in five minutes and at the same moment.

Different kinds of creatures, then, bring with them a good deal of cleverness, and a very useful acquaintance with the established order of nature. At the same time all of them later in their lives do a great many things of which they are quite incapable at birth. That these are all matters of pure acquisition appears to me an unwarranted assumption. The human infant cannot masticate; it can move its limbs, but cannot walk, or direct its hands so as to grasp an object held up before it. The kitten just born cannot catch mice. The newly hatched swallow or tomtit can neither walk, nor fly, nor feed itself. They are as helpless as the human infant. Is it as the result of painful learning that the child subsequently seizes an apple and eats it? that the cat lies in wait for the mouse? that the bird finds its proper food and wings its way through the air? We think not. With the development of the physical parts, comes, according to our view, the power to use them, in the ways that have preserved the race through past ages. This is in harmony with all we know. Not so the contrary view. So old is the feud between the cat and the dog, that the kitten knows its enemy even before it is able to see him, and when its fear can in no way serve it. One day last month, after fondling my dog, I put my hand into a basket containing four blind kittens, three days old. The smell my hand had carried with it set them puffing and spitting in a most comical fashion.

That the later developments to which I have referred are not acquisitions can be in some instances demonstrated. Birds do not *learn* to fly. Two years ago I shut up five unfledged swallows in a small box not much larger than the nest from which they were taken. The little box, which had a wire front, was hung on the wall near the nest, and the young swallows were fed by their parents through the wires. In this confinement, where they could not even extend their wings, they were kept until after they were fully fledged. Lord and Lady Amberley liberated the birds and communicated their observations to me, I being in another part of the country at the time. On going to set the prisoners free, one was found dead—they were all alive on the previous day. The remaining four were allowed to escape one at a time. Two of these were perceptibly wavering and unsteady in their flight. One of them, after a flight of about ninety yards, disappeared among some trees; the other, which flew more steadily, made a sweeping circuit in the air, after the manner of its kind, and alighted, or attempted to alight, on a branchless stump of a beech; at least it was no more seen. No. 3 (which was seen on the wing for about half a minute) flew near the ground, first round Wellingtonia, over to the other side of the kitchen-garden, past the bee-house, back to the lawn, round again, and into a beech-tree. No. 4 flew well near the ground, over a hedge twelve feet high to the kitchen-garden through an opening into the beeches, and was last seen close to the ground. The swallows never flew against anything, nor was there, in their avoiding objects, any appreciable difference between them and the old birds. No. 3 swept round the Wellingtonia, and No. 4 rose over the hedge just as we see the old swallows doing every hour of the day. I have this summer verified these observations. Of two swallows I had similarly confined, one, on being set free, flew a yard or two too close to the ground, rose in the direction of a beech-tree, which it gracefully avoided; it was seen for a considerable time sweeping round the beeches and performing magnificent evolutions in the air high above them. The other, which was observed to beat the air with its wings more than usual, was soon lost to sight behind some trees. Titmice, tomtits, and wrens I have made the subjects of a similar experiment and with similar results.

Again, every boy who has brought up nestlings with the hand

* Read at the Bristol meeting of the British Association.

must have observed that while for a time they but hold up their heads and open their mouths to be fed, they by-and-by begin quite spontaneously to snap at the food. Here the development may be observed as it proceeds. In the case of the swallow I am inclined to think that they catch insects in the air perfectly well immediately on leaving the nest.

With regard, now, to man, is there any reason to suppose that, unlike all other creatures, his mental constitution has to be in the case of each individual built up from the foundation out of the primitive elements of consciousness? Reason seems to me to be all the other way. The infant is helpless at birth for the same reason that the kitten or swallow is helpless—because of its physical immaturity; and I know of nothing to justify the contrary opinion, as held by some of our distinguished psychologists. Why believe that the sparrow can pick up crumbs by instinct, but that man must learn to interpret his visual sensations and to chew his food? Dr. Carpenter, in his "Mental Physiology," has attempted to answer this argument in the only way in which it could be answered. He has produced facts which appear to him to prove "that the acquirement of the power of visually guiding the muscular movements is experimental in the case of the human infant." More than forty years ago Dr. Carpenter took part in an operation performed on a boy three years old for congenital cataract. The operation was successful. In a few days both pupils were almost clear; but though the boy "clearly recognised the *direction* of a candle or other bright object, he was unable as an infant to apprehend its *distance*; so that when told to lay hold of a watch he groped at it just as a young child lying in its cradle." He gradually began to use his eyes; first in places with which he was not familiar, but it was several months before he trusted to them for guidance as other children of his age would do. No one will doubt the accuracy of any of these statements; but I cannot agree with Dr. Carpenter that he had in the case of the boy anything "exactly parallel" to my experiment of hooding chickens at birth and giving them their sight at the end of one or two days. This boy was couched when three years old. Probably sight would have been at first rather puzzling to my chickens, had they not received it until they were six months old. Dr. Carpenter seems to have forgotten for the moment that instincts as well as acquisitions decay through desuetude, and that this is especially true when the faculties in question have never once been started into action and are of the kind which develop through exercise. Another and vital difference between Dr. Carpenter's experiment and mine is this, that when at the end of two days I gave my chickens sight, I did not do so by poking out or lacerating the crystalline lenses of their eyes with a needle.

The presumption, then, that the progress of the infant is but the unfolding of inherited powers remains as strong as ever. With wings there comes to the bird the power to use them; and why should we believe that because the human infant is born without teeth, it should, when they do make their appearance, have to discover their use by a series of happy accidents?

One word as to the origin of instincts. In common with other evolutionists, I have argued that instinct in the present generation may be regarded as the product of the accumulated experiences of past generations. More peculiar to myself, and giving a special meaning to the word experience, is the view that the question of the origin of the most mysterious instincts is not more difficult than, or different from, but is the same with the problem of the origin of the physical structure of the creatures. For, however they may have come by their bodily organisation, it, in my opinion, carries with it a corresponding mental nature.

In opposition to this view it has been urged that we have only to consider almost any well-marked instinct to see that it could never have been a product of evolution. We, it is said most frequently, cannot conceive the experiences that might by inheritance have become the instincts; and we *can* see very clearly that many instincts are so essential to the preservation of the creatures that without them they could never have lived to acquire them. The answer is easy. Granting our utter inability to go back in imagination through the infinite multitude of forms, with their diversified mental characteristics, that stand between the greyhound and the speck of living jelly to which, according to the theory of evolution, it is related by an unbroken line of descent. Granting that we are, if possible, still less able to picture in imagination the process of change from any one form to another. What then? Not surely that the theory of evolution is false! For the same argument will prove that no man present can possibly be the son of his father. Our ignorance is very great, but it is not a very great argument.

The other objection, that the creatures could never have lived to acquire their more important instincts, rests on a careless misunderstanding of the theory of evolution. It assumes in the drollest possible way that evolutionists must believe that in the course of the evolution of the existing races there must have from time to time appeared whole generations of creatures that could not start on life from the want of instincts that they had not got. There can be no need to say more than that these unfortunate creatures are assumed to have been singularly unlike their parents. The answer is, that it is not the doctrine of evolution that the bodies are evolved first by one set of causes and the minds are put in afterwards by another. This notion is but the still lingering shadow of the individual-experience psychology. As evolutionists, whether we take the more common view and regard the actions of animals as prompted by their feelings and guided by their thoughts, or believe, as I do, that animals and men are conscious automata, in either case we are under no necessity of assuming in explanation of the origin of the most mysterious instincts anything beyond the operation of those laws that we see operating around us, but concerning which we have yet to learn more, perhaps, than we have learned.

D. A. SPALDING

SOCIETIES AND ACADEMIES

PARIS

Academy of Sciences, Sept. 20.—*Résumé* of the observations of the sun and of the planets Mercury, Venus, Mars, Jupiter, Saturn, and Uranus, made at the Paris Observatory during the year 1874, by M. Leverrier.—On a remarkable anatomical peculiarity of the rhinoceros, by MM. Paul and Henri Gervais.—Addition to the note relating to M. Bienaymé's theorem, by M. J. Bertrand.—Chemical and spectroscopic characters of a new metal, Gallium, discovered in a blende from the Pierrefitte mine, Argeles Valley, Pyrenees, by M. Lecoq de Boisbaudran. An account of this metal has already appeared in our columns.—Theorem on the composition of co-variants, by M. C. Jordan.—Preliminary note on the function of the protective sheath in herbaceous Dicotyledons, by M. J. Vesque.—On a vertical column of vapour observed from a balloon, by M. W. de Fonvielle.—On the development and structure of interior foliaceous glands, by M. Joannes Chatin.—Existence and development of the *Avicula contorta* zone in the Isle of Corsica, by MM. L. Dieulafait and Hollande.—On the theory of hail, by M. E. Renou.—On hailstones picked up at Criel-sur-Mer during the storm of August 12, 1875, by M. A. Landrin.

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